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365.25

The Origin of Monumental Architecture in Egypt	<i>H. Frankfort</i>	329
The Old Aramaic Alphabet at Tell Halaf: The Date of the "Altar" Inscription - - - - -	<i>Raymond A. Bowman</i>	359
The Story of Jericho: Further Light on the Biblical Narrative	<i>John Garstang</i>	368
Darius and His Egyptian Campaign - - -	<i>Richard A. Parker</i>	373
Hurrian Consonantal Pattern - - -	<i>Pierre M. Purves</i>	378
The Oriental Institute Archeological Report on the Near East, 1941 - - - - -		405
Book Reviews - - - - -		417
J. M. Powis Smith, <i>The Prophets and Their Times</i> (Theophile J. Meek)		
Douglas E. Van Buren, <i>The Cylinder Seals of the Pontifical Biblical Institute</i> (H. Frankfort)		
Richard F. S. Starr, <i>Indus Valley Painted Pottery</i> (Donald E. McCown)		
Books Received - - - - -		427

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CONTENTS OF VOLUME LVIII

ABBOTT, NABIA, Pre-Islamic Arab Queens - - - - -	1
——, Women and the State on the Eve of Islam - - - - -	259
BOWMAN, RAYMOND A., An Aramaic Journal Page - - - - -	302
——, The Old Aramaic Alphabet at Tell Halaf: The Date of the “Altar” Inscription - - - - -	359
BRAIDWOOD, ROBERT J., The Date of the Byblos Temples - - - - -	254
BRUX, ADOLPH A., A Simplified System of Hebrew-English and Ara- maic-English Transliteration - - - - -	57
CAMERON, GEORGE G., Darius and Xerxes in Babylonia - - - - -	314
FRANKFORT, H., The Origin of Monumental Architecture in Egypt - - - - -	329
——, Book Review: <i>The Cylinder Seals of the Pontifical Biblical In- stitute</i> by Douglas E. Van Buren - - - - -	419
GARSTANG, JOHN, The Story of Jericho: Further Light on the Biblical Narrative - - - - -	368
HARDY, ROBERT S., The Old Hittite Kingdom—A Political History - - - - -	177
HYATT, J. PHILIP, A Note on YIWWĀDA ^c in Ps. 74:5 - - - - -	99
IRWIN, W. A., Samuel and the Rise of the Monarchy - - - - -	113
——, A Rejoinder - - - - -	100
JACOBSEN, THORKILD, Book Review: <i>Lamentation over the Destruction of Ur</i> by Samuel N. Kramer - - - - -	219
JONES, TOM, Two Inscriptions of Ashurnasirpal - - - - -	326
KRAELING, E. G., The Origin of the Name “Hebrews” - - - - -	237
MCCOWN, DONALD E., Book Review: <i>Indus Valley Painted Pottery</i> by Richard F. S. Starr - - - - -	421
MAY, HERBERT GORDON, “Al . . .” in the Superscriptions of the Psalms - - - - -	70
MEEK, THEOPHILE, J., Book Review: <i>The Prophets and Their Times</i> by J. M. P. Smith, Revised Edition by W. A. Irwin - - - - -	417
OBERMANN, JULIAN, Koran and Agada: The Events at Mount Sinai - - - - -	23
The Oriental Institute Archeological Report on the Near East - - - - -	104
The Oriental Institute Archeological Report on the Near East, 1941 - - - - -	405
PARKER, RICHARD A., Darius and His Egyptian Campaign - - - - -	373

PARKER, RICHARD A., Persian and Egyptian Chronology - - -	285
PURVES, PIERRE M., Hurrian Consonantal Pattern - - -	378
SPRENGLING, M., Pahlavi Notes - - - - -	169
—, The Berlin Druze Lexicon (<i>continued</i>) - - - - -	91
STAPLES, W. E., The Reading of Hebrew - - - - -	139
THOMAS, L. V., Book Review: <i>Al-Ibānah 'an Uṣūl ad-Diyānah</i> by Walter C. Klein - - - - -	327
TORREY, CHARLES C., A Forged Phoenician Royal Inscription in the Louvre - - - - -	135
WATERMAN, LEROY, Some Repercussions from Late Levitical Genealogical Accretions in P and the Chronicler - - - - -	49
WILSON, JOHN A., The Egyptian Middle Kingdom at Megiddo - -	225
WINLOCK, H. E., Graffiti of the Priesthood of the Eleventh Dynasty Temples at Thebes - - - - -	146
WORRELL, W. H., Four Safaitic Graffiti - - - - -	217
—, Notice of a Second-Century Text in Coptic Letters - - -	84
ZIMMERMANN, FRANK, On Eccles. 3:18 - - - - -	100
BOOKS NOTED:	
Glueck, Nelson, <i>The Other Side of the Jordan</i> - - - - -	328
Grant, Elihu, and Wright, G. Ernest, <i>Ain Shems Excavations</i> , Part V: <i>Text</i> - - - - -	328
Horst, Friedrich, <i>Die zwölf kleinen Propheten</i> - - - - -	102
"Ishmerai," <i>Ezekiel Speaks Today: A Critical Analysis of the Prophecy of Ezekiel in the Light of Modern Events</i> - - - - -	427
Klein, Walter C., <i>Al-Ibānah 'an Uṣūl ad-Diyānah</i> - - - - -	327
Kraeling, Emil G., <i>The Book of the Ways of God</i> - - - - -	102
Kramer, Samuel N., <i>Lamentation over the Destruction of Ur</i> - - -	219
Lacheman, E. R., <i>Miscellaneous Texts</i> - - - - -	102
Langton, N. and B., <i>The Cat in Ancient Egypt</i> - - - - -	224
Lautner, Julius Georg, <i>Altbabylonische Personenmiete und Erntearbeiter Verträge</i> - - - - -	428
Lieberman, Rabbi Saul, <i>Shkiiin: A Few Words on Some Jewish Legends, Customs, and Literary Sources Found in Karaite and Christian Works</i> -	103
Lutz, Henry F., <i>A Neo-Babylonian Debenture and A Recorded Deposition concerning Presentment for Tax Payment</i> - - - - -	427
Marrero, Levi, <i>Perfil del imperio hitita</i> - - - - -	427
Smith, J. M. P., <i>The Prophets and Their Times</i> . Revised edition by W. A. Irwin - - - - -	417
Starr, Richard F. S., <i>Indus Valley Painted Pottery</i> - - - - -	421
Van Buren, Douglas E., <i>The Cylinder Seals of the Pontifical Institute</i> -	419
Van den Oudenrijn, Marc-Antoine, <i>La Vie de Saint Za Mikā'el 'Aragāwī</i>	224
Wright, G. Ernest, <i>See Grant, Elihu</i>	

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THE ORIGIN OF MONUMENTAL ARCHITECTURE
IN EGYPT

H. FRANKFORT

Interchange of ideas between individuals, it is generally admitted, stimulates mental activity. But that intercourse between communities has the same effect is not so readily granted. In fact, any suggestion of foreign influence upon a community is likely to be regarded as derogatory to the group. It is forgotten that the "cultural potential"¹ of the group is one of the most important elements in the process and that there is an immense difference between mechanical copying, on the one hand, and, on the other, creative borrowing in which a stimulus from outside unchains indigenous inventiveness.

The origin of monumental architecture in Egypt is a case in point. Suddenly, with the First Egyptian Dynasty, we find throughout the country buildings of sun-dried brick, ornamented with elaborate recesses. This type of architecture did not survive the Fourth Dynasty, but its derivations, translated into stone or paint, are found at all periods in the "false doors" of the tombs and in the traditional frame

¹ A useful term introduced by Captain G. H. L. F. Pitt-Rivers. See *Race and Culture* (London: Royal Anthropological Institute of Great Britain and Ireland, 1935), p. 4.

The illustrations used for this article are drawn from the following sources: Pl. I, A and B: courtesy the Oriental Institute and Dr. Heinrich Balcz; Pl. I, C, with Figs. 3, 7, and 11: from Petrie, Wainwright, and Gardiner, *Tarkhan*, Vol. I; Pl. I, D, and Fig. 12: from Capart, *L'Art égyptien*, Vol. I; Pl. I, E, and Fig. 4: courtesy E. A. Speiser; Figs. 1 and 2: from De Morgan, *Récherches ... de l'Égypte*; Figs. 5 and 6: from UVB, Vol. VIII; Fig. 8: from *Kemī*, VII (1938), 40; Fig. 9: after Balcz.

which surrounds the Horus name of Pharaoh. The meaning and function of these two features are fairly well known. They are therefore commonly used as the starting-point for a discussion of the earliest Egyptian buildings in brick. But, as we shall see, we can explain neither the character of these latter structures nor the sudden appearance of a highly sophisticated method of brick-building in Egypt in that way. Moreover, any explanation of the facts in strictly Egyptological terms ignores the remarkable circumstance that closely similar buildings were erected at about the same time in Babylonia. Now all these problems find a simple and straightforward solution when we assume that a technique and style which we can watch developing in Mesopotamia became known to the Egyptians about the time of Menes.

Recent Egyptological writings show a noticeable aversion to this sort of explanation. This is largely a reaction against the gross overworking of the argument of foreign influence by a preceding generation of authors. But the prevalent assumption that Egypt, especially in the formative phases of its development, was entirely self-contained is equally unwarranted. Hence this prefatory plea for an unbiased approach to the problem under discussion.

I

Stone architecture, so characteristic for Egypt, was not introduced before the conclusion of the experimental phase of Egyptian culture in the Third Dynasty. Blocks of stone, boulders adapted to a limited architectural purpose, had been used already in the First Dynasty for important parts in the subterranean sections of the royal tombs, such as doorsills, floors, and the portecllis. But the visible portions of the tombs, as well as the cenotaphs and valley temples which formed part of the funerary monuments of Dynasties I-II, were built of sun-dried bricks. They represent the earliest Egyptian buildings in that material and, at the same time, are the earliest monumental buildings known in the Nile Valley. They therefore form a new departure in two respects.

As regards the building-material, there is no difficulty in assuming that the Egyptians invented brickmaking of their own accord. The point cannot be proved, though, for we do not know of any structures

in brick antedating the First Dynasty.² At Maadi some square bricks were found³ (later Egyptian bricks are always oblong), but they did not belong to any recognizable structure. The houses there were built of wood and matting, distinguished from those at Badari and from Amratian houses by a somewhat more solid method of construction. At Merimde dugouts have been discovered, oval structures, half-underground, built up with lumps of clay. In building these, and in daubing wattle and reed shelters with clay, there was ample opportunity to invent brickmaking, which merely required standardization of the lumps used. But opportunity does not always lead to invention,⁴ and there is nothing to show that bricks were used in Egypt to any extent prior to the First Dynasty. Corroborative evidence that the introduction of brick architecture was one of the features distinguishing early dynastic from prehistoric culture comes from Nubia. There the predynastic culture continued to flourish after the reign of Menes, and brick architecture is not found in protodynastic times.

We have evidence that public buildings too were built of wood and matting in predynastic Egypt. The Hunters' Palette depicts one of these buildings, and pictures of others survive in hieroglyphs representing certain traditional shrines or palaces (Fig. 10).⁵ There is, therefore, nothing in what we know of predynastic architecture that would lead up to the brick structures of the First Dynasty. Yet the latter sometimes recall the older buildings in one respect: they occasionally carry a painted decoration which renders poles and multi-

² Petrie and Quibel (*Naqada and Ballas*, p. 54) mention "New Race" graves with brick lining, but there is no certainty that they antedate the First Dynasty. Petrie (*Prehistoric Egypt*, p. 44) gives the sequence dates of these graves as 50-70 and 74, respectively, and sequence dates do not work well for the last part of the predynastic period. Babylonian influence becomes strongly manifest at sequence date 63, where the Gebel el Araq knife handle is to be placed because of the type of its flint blade.

³ *Mitteilungen des Deutschen Instituts für Ägypt. Altertumskunde* (hereafter abbreviated *MDIAA*), V (1934), 112.

⁴ The invention of copper-working for practical purposes is a good example of this lack of connection between opportunity and achievement. A. Lucas has shown (*JEA*, XIII, 162 ff.) that Egypt was most favorably situated to make the discovery, yet comparison of the actual artifacts shows that Asia possessed a far superior copper industry throughout the fourth and third millenniums B.C. and that it was already regularly making objects of practical use, while Egypt produced such objects and some ornaments only occasionally. Copper came into practical use in Egypt by the end of the Predynastic period when intercourse with Asia had demonstrably reached considerable intensity (see Sec. III of this article).

⁵ The interpretation as shrines is usual. That on the lower right of our figure is called a palace by Sethe, *Urgeschichte und älteste Religion der Ägypter* (1930), p. 130, n. 2.

colored mats. There is no example of this painting known in the First Dynasty, when only traces of whitewash or red paint are found. But a few of the later instances show a painted decoration which is obviously intended to equate the structure with the venerable pavilions and kiosks of an earlier age.⁶ The more striking is the contrast between the plan of the new buildings and the requirements of the old material which the painting simulates. We observe the rendering of a wooden framework of uprights braced by horizontal poles; and variegated mats are lashed to the latter by means of ropes (Fig. 12). This procedure leads in actuality to wide unbroken stretches of surface. But the brick wall, upon which the image of this construction is painted in the Old Kingdom, presents a succession of projections and recesses of not more than ten to twenty centimeters in width. There can be no question of any connection between the plan of the brick structures and those which had hitherto been built of wood and mats. The painting is evidently superimposed upon a building of entirely independent origin, and it cannot, therefore, explain the latter at all.⁷

The contrast just discussed underlines the extraordinary fact that there are no antecedents in Egypt for structures of such complexity as the tombs and cenotaphs of the First Dynasty. Of predynastic times none are known to us; the tombs of Aha and Narmer, the rivals

⁶ This is the view most commonly held. Reisner (*Development of the Egyptian Tomb*, p. 292) assumes that the painting imitates a temporary structure erected to harbor offerings or perhaps the king's body while the tomb was being prepared. It would be most unusual to find that the permanent structure simulated the appearance of the temporary one instead of the reverse.

⁷ It may be an accident, but the earliest occurrence of this style of painting dates to the Third Dynasty (Hesire); if not accidental, this fact would suggest a wilful approximation to hallowed archaic structures rather than a mere imitation of a recently defunct style of building. The latter assumption would have been supported if the earliest brick buildings, those of the First Dynasty, had been covered with designs imitating mats on a wooden framework. The importance of this question is that imitation would lead to a faithful reproduction of as many original features as was practicable, while the magical equation did not require an exact reproduction of the original structure, and the existence of the recessed surface of the brickwork would be no objection to covering it with a suitably adapted design suggesting the older technique of building. A. van Gennep and G. Jequier in *Le Tissage aux cartons et son utilisation décorative dans l'ancienne Egypte* (Neuchâtel, 1916) show that the designs on the "false doors" could have been produced by the technique which they discuss. If the "false door" is, however, considered in its relation with the actual buildings of the First Dynasty which have survived and which are recessed all around, it is hard to see how a technique which produces narrow strips of woven material can account for the architectural forms with which we are confronted. (See Sec. II, 4, below.)

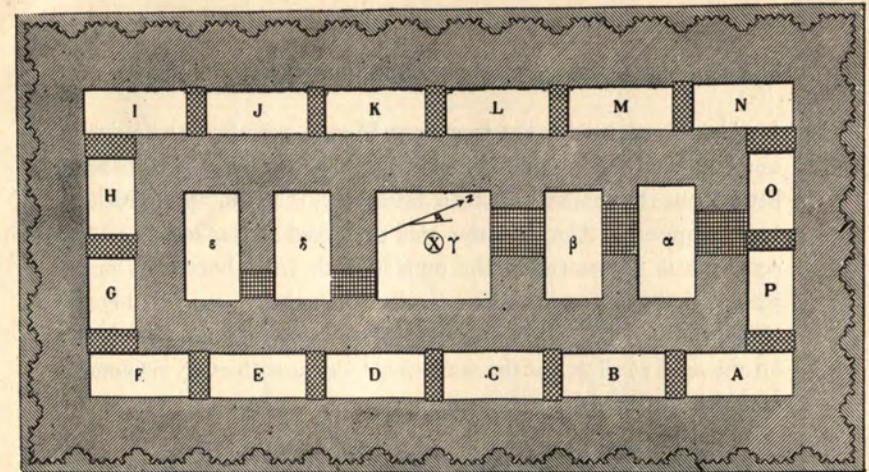


FIG. 1

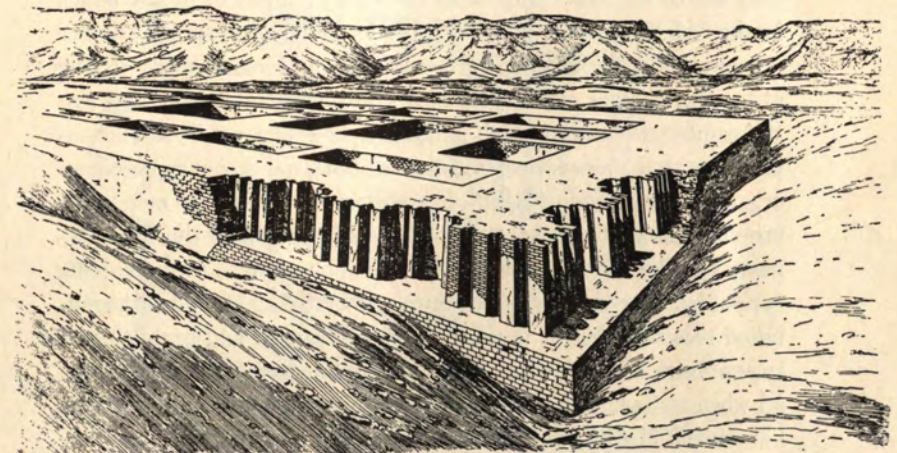


FIG. 2

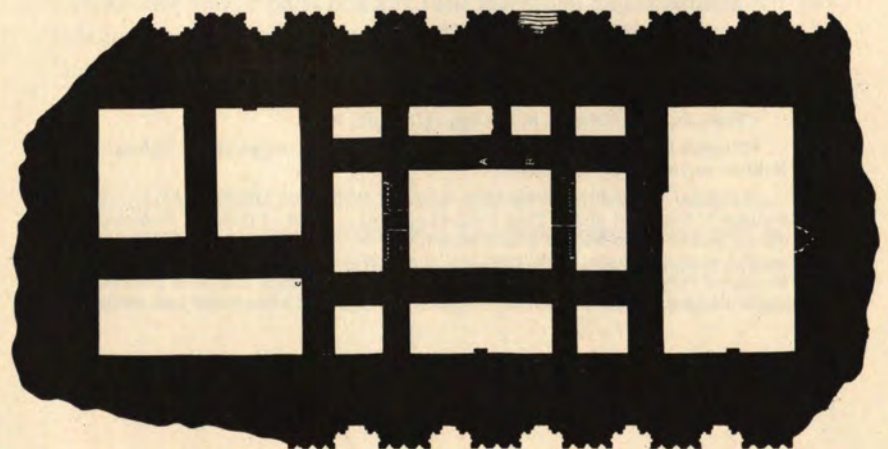


FIG. 3

for identification with the legendary Menes, were lined with bricks and possessed presumably a brick superstructure.⁸ But if we can say little about the character of their brickwork, the cenotaph of Neithotep, the queen of Aha, has been well preserved at Naqada. And this cenotaph is decorated on the outside with an elaborate system of recesses built of special bricks smaller than those used for the core (Figs. 1 and 2). Thus the first generation of Egyptians to use bricks on any scale at all was at the same time familiar with every refinement of which the material was capable.

It has been shown that structures similar to the cenotaph of Neithotep stood at the edge of the cultivation at Abydos, to the east of the royal tombs of the First Dynasty. Petrie has plausibly suggested that they served the same purpose as the valley temples of the pyramids in the Old Kingdom.⁹ Moreover, tombs of this type spread all over the country within the first few reigns of the First Dynasty.¹⁰ The absence of simpler constructions of bricks preceding these, and also the painted decoration which imitates a mode of construction incompatible with the new forms, suggest that the latter do not represent an organic development of Egyptian architecture. It was not merely the use of bricks that appears to have been adopted under the First Dynasty but the use of bricks in a definite application to a very specific type of building, namely, to structures decorated all around with graduated recesses. And it is precisely this advanced and sophisticated type of brick building which is found in Mesopotamia during the period when contact with Egypt is known by a great deal of evidence to have taken place.

A comparison of Figures 12 and 1 with Figures 5 and 6 shows that the Mesopotamian temples of the Uruk and Jemdet Nasr periods present outlines resembling those of the cenotaphs and tombs of the First Dynasty in Egypt. In Mesopotamia, however, the development

⁸ These are fully discussed in Reisner, *op. cit.*, pp. 307 ff.

⁹ Flinders Petrie, *The Tombs of the Courtiers*. His view is indorsed and elaborated by Reisner, *op. cit.*, pp. 10, 243, and 349.

¹⁰ *Naqada*: cenotaph of Neithotep (so-called "Tomb of Menes"); *Abydos*: valley temples of the kings of the First Dynasty; *Saqqara*: tombs of Hemaka, Nebetka, and others, mainly in the reign of Den-Wedimu; Q.S. 2185; *Giza*: Tomb V. Kees (*Kulturgeschichte*, p. 326, n. 3) speaks of "fürstliche" tombs in the north of Egypt, but this must be translated "princely," not "royal," though the context suggests that Kees intended the latter meaning. The monuments are not, however, tombs of kings but of high officials.

of these complicated outlines from simpler plans can be followed in some detail.

Bricks were used in Mesopotamia in the Al Ubaid period, and in the

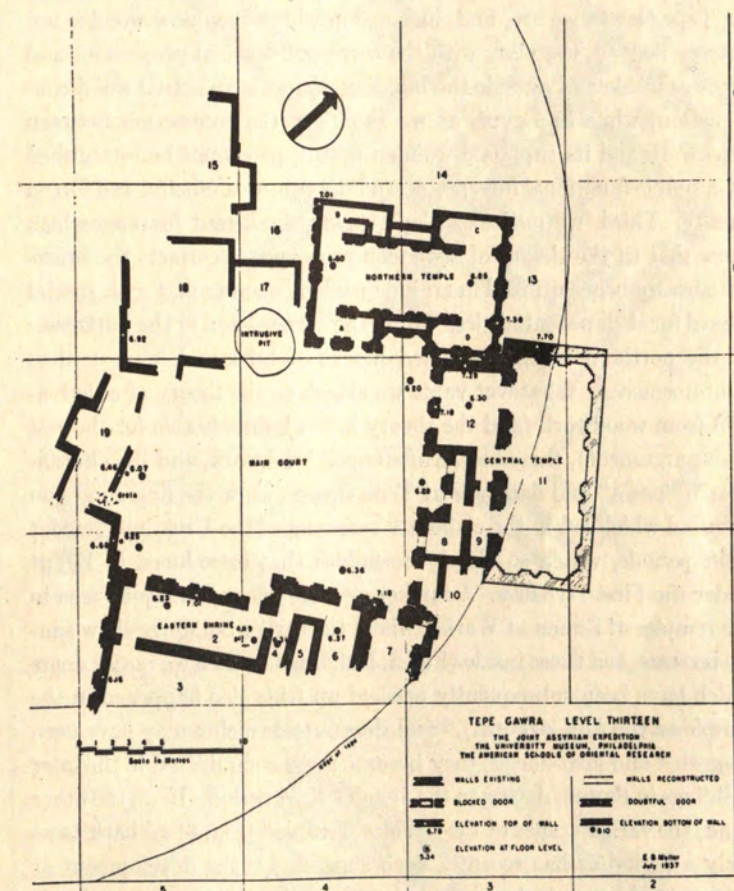


FIG. 4

north, at Tepe Gawra, we find recessing in use even at this early time on the acropolis of Gawra XIII (Fig. 4). This recessing, however, is considerably more simple than we find in Egypt. Lenzen has pointed out that the plans of Gawra XIII may well reflect wooden buildings

in which stout posts are connected by plank walls.¹¹ We must insist at once that this explanation differs in all essentials from the similar-sounding one which has been frequently applied to the Egyptian recessed buildings and which we shall discuss in detail below (Sec. II, 1). At Tepe Gawra we are, first, in a region where even now wood is not scarce. Second, the plan, with the correspondence of projections and recesses inside and outside the building, suggests an actual wood construction, while in Egypt, as we shall see, the connection between brickwork and its supposed wooden prototype cannot be established by a simple functional interpretation but requires a fanciful and forced theory. Third, we notice at Tepe Gawra significant features which show that (if the theory of a wooden prototype is correct) the bricks are already being utilized in an independent manner and with special regard for their potentialities. For in the corners, and in the buttresses of the northern temple, stepped blocks of brickwork were used as reinforcements. Whatever value we attach to the theory of a derivation from woodwork (and the theory is not indispensable for the rest of our argument), these blocks of stepped brickwork, and the alternation of "posts" and flat walls at Tepe Gawra, show the first stages on the road which led to the elaborate recessing of the Uruk and Jemdet Nasr periods, which so closely resembles that introduced in Egypt under the First Dynasty. A further step on this road may be seen in the temples of Eanna at Warka, where the earlier buildings show simple recesses, and these inside (Fig. 5, Building C); then we notice doors which have been subsequently bricked up (this also happened in the temple on the Anu ziggurat),¹² and thus outside niches may have been suggested and introduced; they become more complicated in the later buildings in Eanna, dating to the Jemdet Nasr period. If, on the other hand, the earlier stages of the "White Temple" (Fig. 6) go back to as early a period as has recently been suggested,¹³ the development at Eanna would merely be collateral and the invention of recessing is

¹¹ UVB, X, 26. The sculptured vase base from Khafaje (*OIC*, No. 20, Fig. 27) cannot be quoted in this connection. The "planks" of the side are joined in such a way that rain water would be guided into the house through the cracks. Nor is Lenzen's view that the lower part would represent a stable tenable; the object is part of a numerous series of decorated temple furniture which does not reflect objects from daily life at all.

¹² UVB, VIII, 4.

¹³ Ann Louise Perkins, "Comparative Stratigraphy of Mesopotamia" (unpublished dissertation to appear shortly as an *SAOC*).

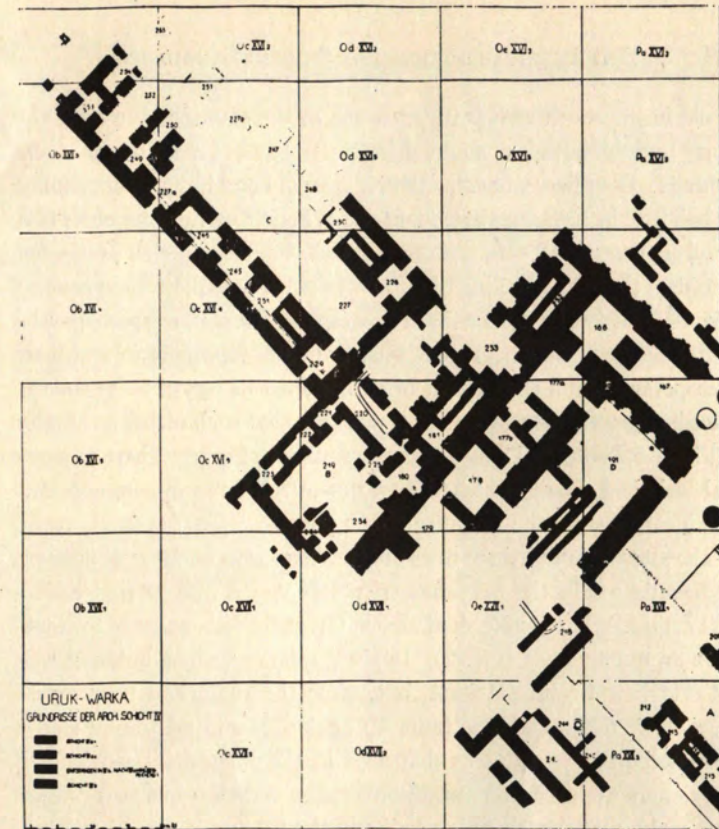


FIG. 5

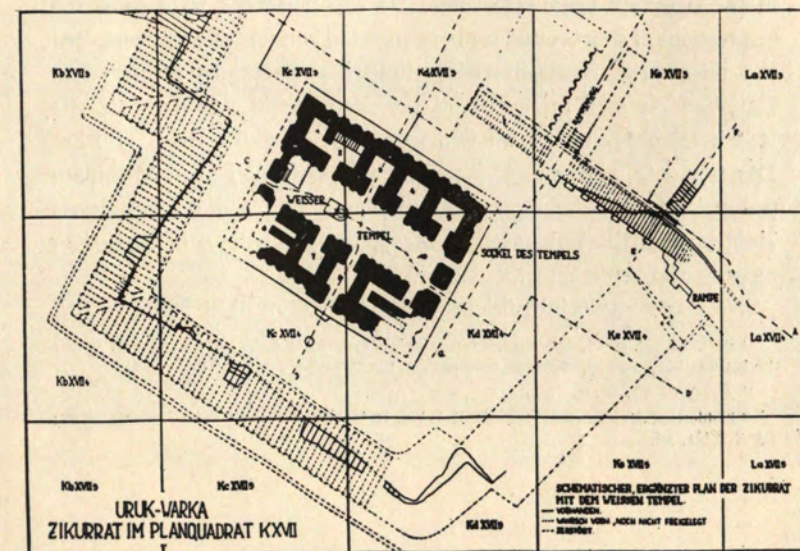


FIG. 6

as old in the south as it is in the north of the country. We need not worry unduly, however, about these early stages of recessing in Mesopotamia. It suffices to realize that it is an old and indigenous manner of building in brick in that country. Whether or not one accepts a wooden prototype for the buildings of Gawra XIII, whether or not one considers that the blocking of doorways helps to explain the presence of outside niches in the south, in any case the close correspondence of details between the developed brick building, especially in southern Mesopotamia, and the earliest brick buildings of Egypt is far too extraordinary to be accidental. It is not only that such niches as appear in Figure 5, Building D, recur exactly similar in Egypt. There are several technical details which the two countries have in common and which corroborate the evidence of the plans.

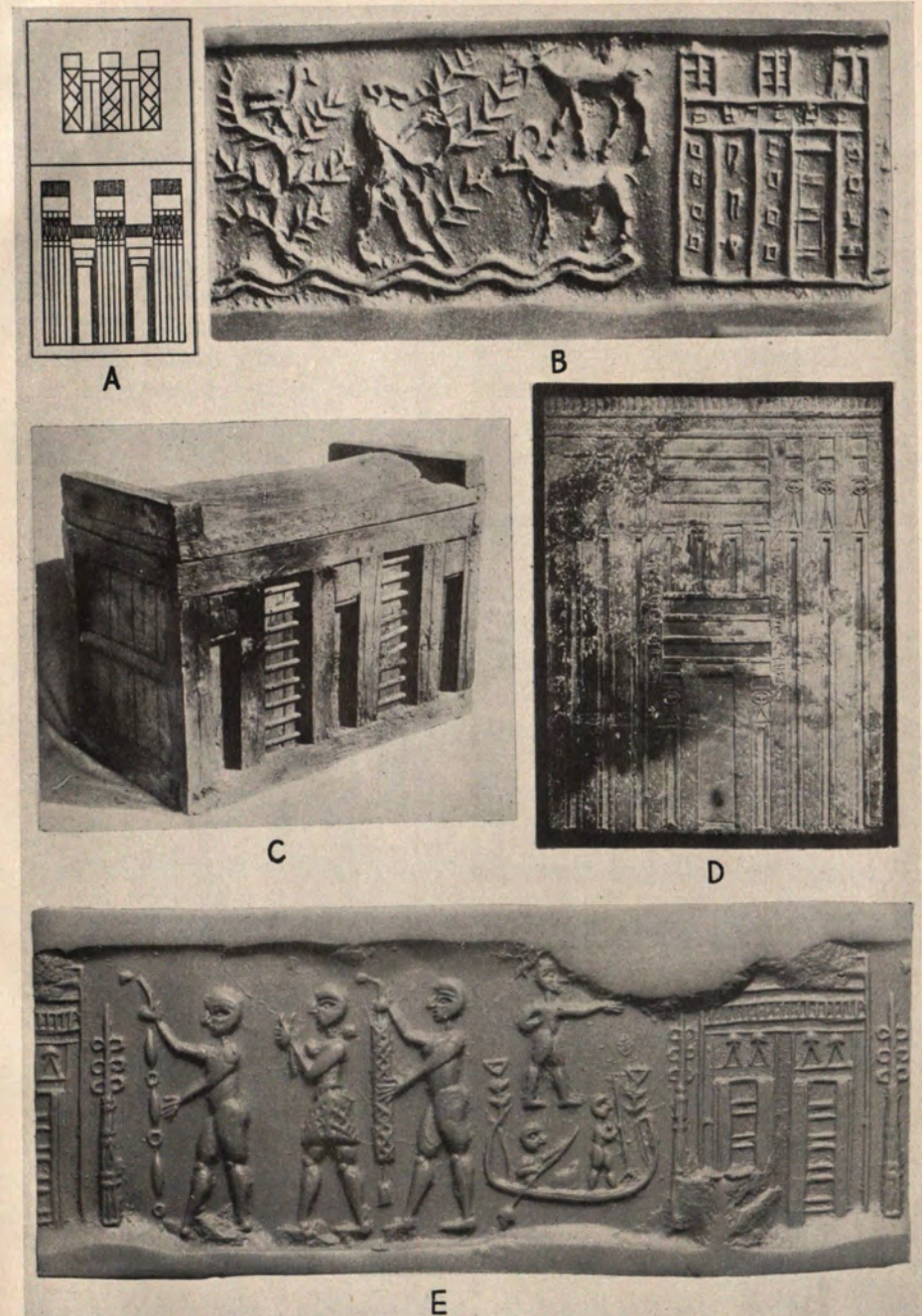
The alternation of three rows of stretchers with one row of headers at Naqada recalls the *Riemchenverband* of Warka and its parallels at Tell Asmar.¹⁴ The cenotaph of Neithotep at Naqada appears to stand upon an oblong base (Fig. 2). In reality, however, the building was not set upon a solid platform, but, after the walls had been erected, a small revetment was built up against it around the outside.¹⁵ This method is the same as that used in the much-discussed *kisu* of Babylonian architecture, first discovered at Babylon but now known from many places such as Ishchali and the "White Temple" at Warka.¹⁶ The latter building possesses yet another feature which recurs in the Egyptian brick structures. Its well-preserved walls show the impressions of short round timbers, inserted horizontally to strengthen the brickwork. Some of these timbers are actually preserved in Egyptian recessed brickwork of the mature Old Kingdom at Abu Roâsh (Fig. 8), but a wooden sarcophagus of Tarkhan, of First Dynasty date, allows us to judge the effect of this feature in the appearance of the older Egyptian buildings (Pl. I, c), and this usage is identical with that observed in Mesopotamia in the combination of recesses and timbers in the "White Temple."

We may recapitulate the argument by stating that building with

¹⁴ OIC, No. 20, p. 11, and Figs. 7-8; Heinrich, *Schilf und Lehm*, p. 40. At Tell Asmar the layers of headers are more numerous than the stretchers.

¹⁵ *ÄZ*, XXXVI, 88, n. 3.

¹⁶ Koldewey, *Das wiedererstehende Babylon*, Index under "*kisu*"; OIC, No. 20, p. 78; UVB, VIII, 38.



sun-dried bricks gradually developed in Mesopotamia to the exact state of complexity in which we meet it, without indigenous antecedents, in First Dynasty Egypt. There is consequently a strong *prima facie* case for assuming that its introduction into Egypt was based on knowledge of contemporary buildings in Mesopotamia. And the corroborative evidence to that effect is conclusive.

To appreciate this, we must realize that only one period in Mesopotamia provides parallels for *all* the features we have just mentioned. That is the Jemdet Nasr period. Only then do we find small oblong bricks used, if not exclusively, then at least predominantly. In preceding ages the bricks were, on the whole, of a much larger type; and in the succeeding Early Dynastic period the bricks were plano-convex. In this latter period the recessing is, moreover, of a different character; if used all around a building, it is of a highly simplified type, while the complicated recesses resembling those in Egypt are reserved for the two towers which flank and thereby accentuate the entrance into palace or temple complex. But in the Jemdet Nasr period we find all-around recessing used outside the buildings and also inside in important rooms or courts such as Room 252 in Figure 5 or Sin Temple V at Khafaje. The extant brick buildings of the First Dynasty do not contain such rooms or courts, but we find the same usage in some temporary buildings of the Fourth Dynasty, erected in brick where the permanent structures were to be of stone.¹⁷ We know that the stone architecture of this dynasty did not use recessing. Its appearance in the brickwork with which Shepseskaf completed the unfinished valley temple of Mycerinus shows, therefore, that recessing was an established traditional feature of brick-building in Egypt at the time and that there is some likelihood that it was used around courts or rooms, just as it was used around the outside of buildings, at the time of its first introduction. This presumed similarity would agree, in any case, with those more firmly established indices pointing to the Jemdet Nasr period (to the exclusion of the periods preceding and following it) as the stage in Mesopotamian culture in which Egypt learned the use of bricks for monumental buildings.

¹⁷ Reisner, *Mycerinus*, Plans IV, VIII, IX and Pls. 31c, 32b, c, 33, and 74. For Mesopotamia see Room 252 of Building C in Fig. 5 and the central room of Sin Temple V in *OIC*, No. 20, Fig. 26.

This conclusion is corroborated by two independent sets of evidence. In the first place, it has long been known that a number of features of pre- and protodynastic Egyptian culture possess Mesopotamian parallels; these can now be assigned to the Jemdet Nasr period. In the second place, there is independent stratigraphical proof from Tell-Judeideh that the Jemdet Nasr period was contemporary with the Late Predynastic and Protodynastic period in Egypt.

We shall consider these two sets of evidence in Section III of this paper. For the moment we merely point out how much material falls into place, how many phenomena find an explanation, when we interpret the sudden appearance of brick architecture in an accomplished form under the First Dynasty as due to Mesopotamian examples. We may well ask whether any alternative explanation can compare with it in coherence and economy of means.

II

All earlier explanations of the recessed buildings of Egypt are defective in that they do not account for the remarkable resemblance which the structures show with contemporary buildings in Mesopotamia. We repeat that this resemblance requires explanation no less than the connections with material falling traditionally within the scope of the Egyptologist. Since, however, these Mesopotamian parallels have been discovered only recently, we shall here consider—on their merits, irrespective of their common failing—the current theories on the origin of this type of structure. They may be classed under four headings. The first two are entirely without foundation; the last two are valuable in establishing the relation between the protodynastic and later usages without, however, explaining how recessing first came to be adopted.

1. In 1912 Flinders Petrie found at Tarkhan sarcophagi made of planks which had originally served another purpose, as shown by a series of holes which stood in no relation to the function of the planks as parts of coffins but which enabled them originally to be lashed to other boards (Fig. 11). Petrie, knowing the unheralded appearance of recessed brick-building in the First Dynasty and having previously entertained the idea that there might be wooden prototypes for these buildings, attempted to join the boards, reused in the Tarkhan coffins,

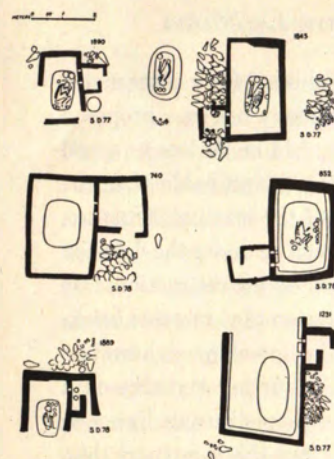


FIG. 7



FIG. 9

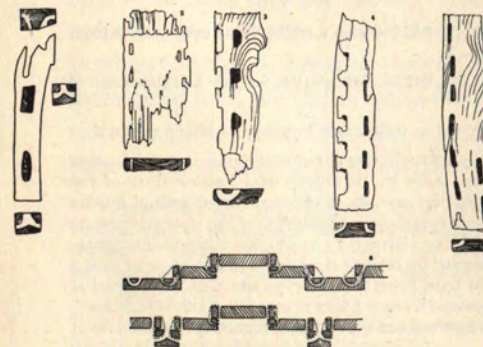


FIG. 11

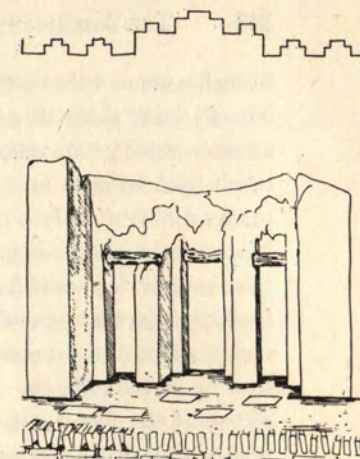


FIG. 8

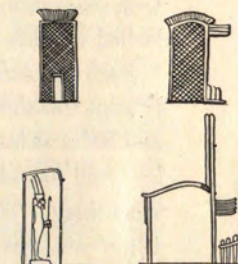


FIG. 10

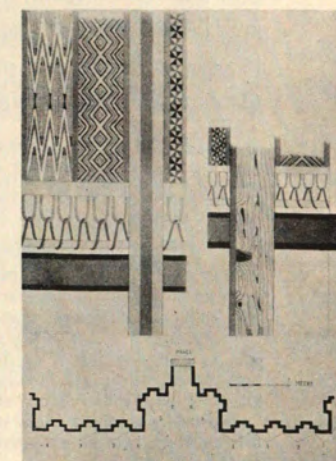


FIG. 12

in such a manner that a recessed surface resulted. Many other methods of joining them are possible, although nobody has ever proposed an alternative combination; and indeed it would be useless to spend much time on such an attempt since it is most probable that the planks represent only a minute remainder of the original structure. Moreover, no one reading Petrie's account¹⁸ can be under the delusion that he provides proof for the correctness of his reconstruction. He frankly states that he *started* with the assumption that recessed brick-building rendered a wooden prototype; he then proclaimed, however, that he had found "the actual timbers." He further embarks on a sketch of the life of "the more prosperous people" using "wooden houses which had to be moved twice a year"; but this is no more than an ingenious fantasy in which puzzling ancient remains are combined with observations of modern Egyptians, though these are innocent of lashed wooden houses.

Such an imaginative reconstruction is often enlightening. In the present instance, however, it has served us ill. Newberry in 1923¹⁹ and Balcz as late as 1929²⁰ adopted Petrie's flight of fancy, and Woolley even introduced it, lock, stock, and barrel, into Mesopotamian archeology.²¹ It is not remembered that the painted decoration (Fig. 12) of this type of building in Egypt, even if it were not incompatible with the wall surface upon which it is superimposed, would not represent a wooden original at all but a framework of poles to which mats are lashed. The building thus rendered agrees perfectly with the pictures of early buildings preserved in hieroglyphs (Fig. 10) and on such monuments as the Hunters' Palette.²² The whole assumption that

¹⁸ Petrie, *Tarkhan I and Memphis V*, p. 24. See also a coffin lid of such planks from Abu Ro'ash in Kemi, Vol. VII, Pl. XIII, 2.

¹⁹ Presidential address to Sec. H of the British Association for the Advancement of Science, 1923, p. 17.

²⁰ *MDIAA* (see above, n. 3), I, 78 ff.

²¹ Hall and Woolley, *Al Ubaid*, pp. 68 ff.

²² Professor E. Baldwin Smith, *Egyptian Architecture as Cultural Expression*, approaches our problem as an architect but does not solve it. He admits that Petrie's story of the wooden houses is unsubstantiated (p. 33) but accepts as relevant his scheme of joining the coffin boards from Tarkhan (p. 47) and postulates a prototype for the *serekh* consisting of a palace constructed of small-wood joinery. Even if we accept the author's interpretation of one of the archaic buildings preserved on seals of the First Dynasty as proof that a "light timbered construction" existed in Late Predynastic Egypt and that it consisted of "a paneled framework made of small pieces of wood skilfully squared and joined together," this would not in any way explain the recessing nor the fact that recessing appears first of all in brick buildings.

wooden houses existed at all is based on the finding of some perforated boards in secondary use. The perforation must, of course, be explained. But we know of a most important class of products which were demonstrably built of lashed and pegged timbers. These are the ships of the ancient Egyptians.²³ It is interesting that some of the boards at Tarkhan are curved (Fig. 11), most unsuitably if they were lashed together to resist climatic changes and to protect the inhabitants of movable wooden houses from "gaps which let the wind blow directly in," but very understandably if they derive from boats. The most natural explanation of the Tarkhan boards is, evidently, that they represent valuable raw material salvaged from wrecked or disused Nile craft which was unsuitable for use in furniture for the living by reason of the holes but which served well enough for coffins.

2. According to certain Egyptologists, the recessed brick buildings present a Lower Egyptian style. This assumption does not explain their origin, of course. Balcz and Newberry, as we have seen, borrow Petrie's hypothesis to account for them, but the first uses one other argument, namely, that in some renderings of these structures a pair of bound papyrus flowers appears high up in the niches (Pl. I, D). This design is considered a parallel of the later symbol in which the hieroglyph *sm*^c, "to unite," joins the heraldic plants of Lower and Upper Egypt. Much ingenuity is spent on the explanation of the fact that two identical plants combined may stand for the combination of two different regions. The entirely hypothetical course of events postulated by Sethe,²⁴ on the strength of later texts (treated with a method of most dubious validity²⁵), is made to supply the political units, the coalescence of which is reflected in the two bound flowers. It seems to be quite forgotten that we lose here every connection with ascertained fact, even irrespective of the circumstance that we know next to nothing about the Delta in prehistoric times but a great deal about Mesopotamia during that period.

²³ Short wood only being available, the planks of the hull were very thick and joined by large pegs in the shape of dowels which would well fit the holes which we observe in the boards. Lashing was also resorted to (see W. F. Edgerton, "Ancient Egyptian Ships and Shipping," *AJSL*, XXXIX, 109 ff., esp. 128 and 134).

²⁴ Sethe, *op. cit.*

²⁵ Kees, in *Nachrichten der Gesellschaft für Wissenschaften in Göttingen* (Phil.-hist. Klasse, 1930): *Kultlegende und Urgeschichte*.

We might, perhaps, drop the matter here, but the distinction of Upper and Lower Egyptian features assumes the characteristics of a fashion and should be warned against. Its origin may be found, perhaps, in Scharff's successful explanation of the puzzling overlapping of the First and Second Predynastic (Amratian and Gerzean) cultures.²⁶ He made it appear highly probable that the Gerzean culture existed in Lower Egypt while the Amratian still continued in the Upper Nile Valley. The distinction between Upper and Lower Egyptian styles of architecture has, however, no evidence in its favor. Yet we find Steckeweh, in an excellent publication,²⁷ contrasting the "open" architecture of Upper Egypt, which tends to elaborate its outward appearance, with the "closed-in-on-itself," introspective style of the North. Wolf²⁸ and Pflüger,²⁹ on the other hand, find the Upper Egyptian style, with its "grave greatness," a "truly African" phenomenon, while the "charming gay lightness of the Mediterranean world" is said to be clearly felt in the Lower Egyptian style. This swamp of fortuitous and contradictory interpretations has even brought some very distinguished authors under its spell. Junker refers to our tombs as a Lower Egyptian type;³⁰ Scharff³¹ admits that the cenotaph of Neithotep at Naqada, in Upper Egypt, is the earliest example of recessed brick-building but insists, nevertheless, on its Lower Egyptian character. The Lower Egyptian style would, besides recessing, be characterized by the inclusion of magazines or other rooms in its superstructure, while the Upper Egyptian type would use smooth outer wall for its solid superstructure but possess subterranean rooms approached by a shaft. If this were correct, it would be paradoxical that in the tomb with two "false doors" it is not the northern but the

²⁶ JEA, XIV, 261 ff.

²⁷ *Die Fürstengräber von Qau* (1936), p. 43. The place which the tombs at Qau occupy in the history of art is very well established by the author. I merely take exception to certain of his interpretative generalizations.

²⁸ *ÄZ*, LXVII (1931), 129 ff.

²⁹ JEA, XXIII, 7 ff.

³⁰ Junker, *Giza II*, p. 6. Daum (in *Giza I*, pp. 66 ff.) postulates a contrast between the Naqada and the Abydos types of tombs which can hardly be substantiated, since the superstructures of the Abydos tombs are unknown. Moreover, he ignores the existence of the valley temples at Abydos.

³¹ *Handbuch der Archäologie*, pp. 440 ff. When Scharff mentions only the tomb of Neithotep at Naqada as an Upper Egyptian example of the supposed Lower Egyptian type, he ignores the valley temples of kings Zer and Zet and Queen Merneith at Abydos.

southern one that shows the recesses in most cases.³² However, the distinction does not hold good. At Sakkara tombs have recently come to light which belonged to high officials, Hemaka and Nebetka, of kings of the First Dynasty, especially of Den-Wedimu.³³ And in these tombs we find recesses but also rooms in both substructure and superstructure.

It would seem that the attempt to distinguish between Upper and Lower Egyptian peculiarities is, in any case, unlikely to lead us very far. The physical configuration of the Nile Valley makes for a uniform culture. What evidence we have seems to show that fashions set by the Residence spread easily and quickly throughout the land except in times of disturbance.³⁴ We can distinguish metropolitan and provincial styles; but, if there were at any time marked local differences between the north and the south, they seem to have become unrecognizable at this distance of time.

Rejecting this theory, we must account for the group of the two joined papyrus flowers high up in the recesses, just as we have accounted for the perforations of the coffin boards at Tarkhan after rejecting Petrie's hypothesis. In the present instance we can be less certain of the correct interpretation, for the flowers preserved are only in ancient renderings, not in the original. It is interesting, in the first place, that a very similar design appears in a Mesopotamian rendering of a recessed building, namely, on a seal cylinder of the Uruk period from Tell Billa (Pl. I, E). Here there can be no question of papyrus, of course; but, if the Egyptians took over certain fairly complicated types of buildings, they may have retained the detail of the flower design but translated it into familiar forms. Its simplest explanation would be a wooden window grille. That would explain quite naturally its position high up in the recesses. In stone models of similar recessed buildings found at Warka,³⁵ the windows seem to be triangular; but that is certainly unusual, and to assume that the pair of flowers in the frame render a square space closed by a grille with an attractive de-

³² Reisner, *Tomb Development*, p. 249.

³³ W. B. Emery, *The Tomb of Hemaka*; JEA, XXIV, 243; *Annales du service des antiquités*, XXXVIII (1938), 455 ff.

³⁴ Brunton (*Qaw and Badari*, I, 75) shows how the spread of certain types of pottery and other objects was interrupted by the upheaval of the First Intermediate period.

³⁵ *UVB*, Vol. VII, Pl. 48.

sign does not tax our imagination unduly. The design is found in Egypt used for this very purpose; it is shown in the Sixth Dynasty in the carved wooden screens which close the curved top, front and back, of Ipi's litter,³⁶ and it recurs in the open-work (*mushrabiya*) upper parts of doorways in the New Kingdom.³⁷

3. It has been assumed that the recessed buildings render a royal palace.³⁸ Not that any palaces anterior to the Eighteenth Dynasty have been discovered, but the framework in which the king's Horus name is written is also known as a hieroglyph reading *serekh*, perhaps best translated "seat of royalty." The *serekh* design shows indeed a panel of recesses; but it is a design on the flat and consequently teaches us very little about the actual appearance of the building which it is supposed to depict. It may be well to recall that no satisfactory correlation has as yet been established between the ruins of the three buildings, probably palaces, actually excavated at Tell el Amarna, and the two detailed drawings of the royal palace which have been found in the rock tombs at the same site. In dealing with the *serekh* we have much less to go by. If we try to translate this design back into three dimensions, we have no guaranty whatsoever that our reconstruction hits the mark. We do not even know which part of the palace is indicated by the *serekh* design. Why should it be the façade and not, for instance, part of the throne-room? Moreover, if we accept the interpretation as façade, we gloss over the very real difficulty that the actual buildings which we know—namely, the cenotaphs and valley temples of the First Dynasty—are covered with recesses on *all four walls*. The idea of a façade picture is fortuitous.

The *serekh* sign actually supports our view of a Babylonian origin of recessed brick-building. If it is hazardous to argue from an ancient rendering to the appearance of the building in the round, we may at least compare Egyptian abbreviated renderings with similar renderings from Mesopotamia. These are preserved on seal cylinders of the Uruk and Jemdet Nasr periods (Fig. 9, *right*). It is quite clear that they resemble the Egyptian designs (Fig. 9, *left*) so closely that some

³⁶ Wreszinski, *Atlas*, I, 405.

³⁷ *ÄZ*, LXXIII, 68 ff. with Pl. VIIIa and Fig. 3.

³⁸ The fullest account of the reasons which support the identification with a royal palace are given in Reisner, *Development of the Egyptian Tomb*.

relation must exist. There are, however, sufficient differences to make it impossible to assume that these Mesopotamian designs themselves were copied in Egypt. Consequently, the actual buildings, of which the Egyptian and Mesopotamian engravings are independent abbreviations, must have resembled each other; and this is the conclusion reached in Section I of this paper.

I am tempted to indulge here in a little speculation. If the tomb, and possibly the palace, of pharaoh assumed architectural forms which in Mesopotamia (whence these forms were derived) were characteristic for temples, this choice of the Egyptians is perfectly understandable, for it corresponds exactly with the difference in Mesopotamian and Egyptian views as to the nature of kingship. The Mesopotamian ruler, though representing the god, was LÚ.GAL, "the Great Man"; but pharaoh was a god.

However this may be, it does not affect the present argument that the derivation of the recessed buildings of the First Dynasty from the royal palace is unsatisfactory because it merely displaces the problem to a category of buildings of which no example survives. The brick buildings in Egypt, tombs and cenotaphs, show recesses on four sides, corresponding in that respect to contemporary Mesopotamian temples. The *serekh* designs are no certain guide to the actual appearance of buildings, but in so far as they resemble contemporary small-scale drawings from Mesopotamia they suggest that somewhat similar buildings were erected at about the same time in both countries. The occurrence of towers raises a fine point of chronology. In the first place, the tower with a vertical side is a regular feature of Mesopotamian architecture but a very unusual one in Egypt which developed the pylon, with a pronounced batter. Second, these towers do not occur on seals of the Uruk period but are known on a cylinder of the Jemdet Nasr period (Pl. I, *B*) and are a standing feature in the succeeding Early Dynastic period. The occurrence of *serekh* designs with towers (Pl. I, *A*) and without (Fig. 9, *left*) suggests therefore, again, the Jemdet Nasr period as the age of contact.

4. We can say that recessed architecture is a feature of brick-building in essence as well as in incidence. The point is strikingly illustrated by some temporary brick structures with which Shepseskaf

completed the unfinished valley temple of Mycerinus.³⁹ Recesses were not used in the stone architecture of the period; yet as soon as bricks were used in a funerary monument they reappear, obviously because they were a traditional method of enriching brick surfaces. In the stone-built tombs of the Old Kingdom we find, however, recessing in one particular instance: the "false door" which forms a feature of these tombs in one of its two usual forms consists of a monolithic slab showing a door flanked by a standardized set of recesses on either side (Pl. I, D). The other form, a narrow doorway in which the figure of the dead appears sometimes underneath a stone imitation of the rolled mat with which it could be closed,⁴⁰ need not be considered in this context.

It is clear that the elaborate form of the false door, the "great door" or "palace façade door," is related to the brick architecture of the First Dynasty; moreover, it is sometimes covered with a painted decoration⁴¹ which is similar to that used in some tombs with recessed walls (Fig. 12). But what is that relationship? Because the false door is a typically Egyptian feature of the tombs, well known and long in use, Egyptologists have often been inclined to make this the basis of an explanation of the First Dynasty tombs with recesses. This procedure is, however, illegitimate.

The "false door" or "Ka-door" served to mark the place where offerings to the dead were made and where, in a general way, communication with the departed took place. This idea is thoroughly Egyptian. In fact, it seems to go back to the broadest Hamitic substratum of Egyptian civilization, since North African tumuli have a niche which seems to fulfil the same purpose.⁴² Some very simple graves of the First Dynasty at Tarkhan (Fig. 7) have a pair of slits in the brick

³⁹ See n. 17.

⁴⁰ E.g., tomb of Mereruka at Saqqara, or Iduw at Gizeh (*Boston Bulletin*, XXIII, 13).

⁴¹ The best-known example is N. de Garis Davies, *The Mastaba of Ptahhetep and Akhetetep*, Pls. XIX and XX-XXA.

⁴² Baumgaertel, *Dolmen und Mastaba* (1926), Figs. 4 and 7. Balcz's objections, endorsed by Wolf, are quite irrelevant. Of course, there is no direct connection between the prehistoric African stone buildings with Egyptian stone architecture. What matters, however, is the common idea underlying both—namely, that the structure of the tomb should mark in some way how communication with the dead is maintained; and, furthermore, the specific architectural form given to that idea in the arrangement of a niche in the tomb.

wall of their superstructure in front of the face of the dead, and the tomb chapel where the offerings are made is constructed at this place against the tumulus. An earlier expression of the same idea may be found in a burial jar from Maadi pierced by two holes.⁴³ A later instance is the serdab in Zoser's funerary complex at Saqqara, which shows two slits in front of the statue's face. Again, in the tombs of the Fourth Dynasty at Gizeh there are perforations in the large stone which closes the entrance to the burial chamber, and behind these openings is placed the "reserve head" of the dead.⁴⁴

All these cases evidently present expressions of the same idea which found its final form in the "false door." Even the fact that two of these doors are often found may well go back to the very primitive use of two slits corresponding with the two eyes. The presence of two doors is generally explained as an imitation of royal usage. The king of Upper and Lower Egypt would have required two false doors in his tomb. But two false doors are found already in tomb Q.S. 2105 at Saqqara, a private tomb of the First Dynasty (reign of Zer), and it seems hazardous to project the adoption of royal usages by private people, traceable from the Late Old Kingdom onward, back into the very period when the united kingship arose. The derivation from a pair of eyeholes of a prehistoric period seems less forced, and the retention of both doors (even after they had become large structures, placed at opposite ends of the wall) would be a characteristic manifestation of that curious Egyptian, or rather Hamitic, tendency toward a dualistic scheme of things.⁴⁵

Whether we find one or two false doors in any given tomb, whether

⁴³ MDIAA, V, Pl. XIX b 1.

⁴⁴ Junker, *Giza I*, p. 43.

⁴⁵ The dichotomy and polarity which Meinhoff (*Die Sprache der Hamiten*, p. 20, n. 1, *et passim*) found in the structure of Hamitic languages is also noticeable in such features as the consistent preservation, beyond all actuality or reason, of the view of Egypt as "the Two Lands"—whatever the historical basis of that designation may have been originally. It appears, moreover, in the manner in which the conflict between Horus and Set becomes the pretext for an exceedingly farfetched system of identification of a number of objects with one or the other of these protagonists (in which Osiris may take the place of Horus, of course). With this in mind a scene like Sethe, *Festspiel*, 141 ff., becomes most illuminating. It illustrates to a nicety the "polarity" which Meinhoff observed in Hamitic grammar. The relation between Ka and man, with the respective physical substrata, such as we find also in the treatment of body and placenta in Uganda (*Man*, 1911, pp. 97 ff.), and similar features, all deserve study as a manifestation of this curious fundamental dualism in the minds of the Egyptians.

they are of the simple or of the elaborate type, it is certain that they marked the place of communication with the dead. No architectural form could be less suitable to serve that purpose than the all-around recessing of the brick tombs of the First Dynasty. It is impossible to explain this type of building as a development of the "false door." The latter marks one particular spot as important; the other presents a series of identical features rhythmically articulating the whole of the available wall surface.

It has been suggested that the all-around recessing expressed the idea that the dead was entirely free to "come forth" and go wherever he wanted.⁴⁶ Even if the Egyptians of the First Dynasty had interpreted the complete recessing in this manner, it would leave us with the unsolved problem of their sudden ability to erect highly sophisticated structures of a material not used by them hitherto or, at least, not used on any considerable scale. On that assumption it would be easier to imagine that some such interpretation made foreign buildings of recessed brickwork attractive models for them to follow. But even this hypothesis has little in its favor, for we have no need to invoke the interpretation of "Ka-doors" to explain the appearance of recessing in Egypt. We have seen that many technical details suggest that it was in this accomplished form that the large-scale use of bricks became known in the Nile Valley.

Moreover, this symbolical function of marking the place of exit of the dead is by no means the most important role which the "false door" plays in tomb architecture. It marks the place of communion with the dead and has, therefore, its significance for the living as well as for the deceased. The perforations in the funerary pot found at Maadi, and those in the stones closing the burial chambers of the Fourth Dynasty tombs, are examples of purely symbolical features, since they appear in places to which the living did not have access. But in the superstructure of the tomb we must expect the main features not only to symbolize the status of the dead (namely, his ability to "go forth") but also to be related to the acts with which the living maintain their relationship with him. Their part in the communion also requires tangible expression. The two slits in the simple First Dynasty tombs of Figure 7 are as much determined by the require-

⁴⁶ Reisner, Balcz, and others.

ment of the celebrants of a formal indication in the structure itself that their acts and words are addressed to the relative buried within, as by the thought of the corpse's two eyes. But the "false door" represents the mature expression of this twofold significance of the place of communion. Standing out in the unbroken wall, it formed as compelling a focus for the ritual as the altar in a church. In its form as "door" it continually stimulated in those gathered in front of it the consciousness that they addressed one mysteriously surviving death. The impenetrable "Ka-door" is magnificently adequate to its function.

But the same reflection which reveals the adequacy of the "false door" shows that its efficacy would be diminished if it were placed within a wall composed of a series of ornamental recesses. If we should accept the theory that recessing is derived from the "false door" and that each recess in fact represents a possible place of exit for the dead, then we are led to the absurd assumption that the ancients had chosen to enact their ritual in front of a partition resembling a modern row of telephone booths—hardly conducive to the concentration of thought and feeling which the occasion demanded. We shall see that one hybrid form of this type is known, but no conceivable development leads from the "false door" to the recessed monuments of the First Dynasty.

Moreover, the latter antedate the appearance of the "false doors" by several reigns. If the underlying *notion* of the "Ka-door" is an immemorial part of Hamitic funerary requirements, its *form* is evidently derived from the recessed architecture which appears, ready made, at the very beginning of the First Dynasty. Thus the relation between "Ka-door" and recessed brick buildings is exactly the reverse of that usually assumed. In fact, we can follow in some detail how the Egyptians adapted the new architectural style to embody the idea of the "Ka-door" until, at a later time, they abandoned recessing as altogether unsuitable. We have at least two instances where we observe how the Egyptians, confronted with a building ornamented all around with identical and equivalent niches, singled out one to serve as a place of communication. At Tarkhan this has been accomplished by putting a wooden floor in one of the niches (Fig. 3). At Naga ed Deir one of the niches is more elaborate than the others, and the deepest

recess is painted red to indicate the wooden leaf of a door.⁴⁷ Both tombs belong to the First Dynasty.

These attempts to superimpose the "false door," required by indigenous custom, upon an architectural form which is characterized by the absence of emphasis on any one point of its circumference are easily explained by our view that Mesopotamian buildings, like our Figures 5 and 6, served as prototypes. For these were neither tombs nor cenotaphs but were accessible through doorways placed in some of the recesses. The Egyptians needed merely to make one of these doors into a "Ka-door." But it is interesting to note that this style of architecture was soon given up in Mesopotamia too, and evidently for the same reason as it was abandoned in Egypt. It was apparently felt to be unsatisfactory that the door, a feature of outstanding functional importance, did not receive any architectural emphasis but was hidden in one among several recesses. This interpretation of the change is no mere modern postulate, for the new Mesopotamian style, appearing by the end of the Jemdet Nasr period and characteristic for the Early Dynastic age, made the entrance the most striking detail in the building's silhouette. The all-around recessing was very much simplified, and the elaborate niches were reserved for the towers, which now flanked the entrance and thus accentuated its presence in the wall. The "Ka-door" required no less adequate architectural expression than the real doorways of the Mesopotamian temples, and it is clear that the makeshifts of Tarkhan and Naga ed Deir could not permanently satisfy the Egyptians. They found the solution in one or two "false doors" standing out in an otherwise unbroken wall surface.

The form of the "false door" derives from that of the early brick recesses, at least in the case of the "palace façade" variety (Pl. I, D), but it is important to realize that, architecturally, the tombs with "false doors" represent the total abandonment of recessed building. There is no gradual development from the one to the other as is so often assumed,⁴⁸ but, on the contrary, we observe the victory of one form over another which had descended from the very first monumental buildings erected (as far as we know) in the Nile Valley. The form

⁴⁷ Reisner, *Tomb Development*, p. 292.

⁴⁸ So Junker in *Giza II*.

with two "false doors" is known already in the First Dynasty,⁴⁹ and Reisner has proved that it remained continuously in use until it became the prevalent type in the mature Old Kingdom.⁵⁰ In other words, the method of all-around recessing is simply superseded.⁵¹

5. The issue is somewhat confused by the existence of a few tombs which show the old recessing, though on one wall only. And this wall is, moreover, mostly part of an inner corridor and not an outside wall. The tomb of Hesire at Saqqara is the best known of these atypical tombs, and there are some of Fourth Dynasty date,⁵² while in the provinces (notably at Denderah) they enjoy a local vogue down to the end of the Old Kingdom.⁵³ The first appearance as well as the distribution of this type shows that it is not part of the main stream of architectural development. It is not transitional but hybrid. The recessed wall in Hesire's tomb contains in the depth of each recess a finely carved wooden panel showing the standing or striding figure of the dead man. Since Egyptian relief does not distinguish between the two attitudes, it is possible to maintain that he is supposed to come forth from each recess. The disadvantages of such a fiction were discussed above, and, in any case, the need to mark clearly the focus of the ritual enacted in the tomb existed here, too, and was met by showing the dead man, not standing, but seated at his funerary meal, in the central recess of the wall. The difference from the other recesses is, of course, too slight to be really adequate; it seems that Hesire had been desirous of combining the rich effect of the recesses with the "Ka-door" as the place where the dead and the survivors communicated, though the two requirements are incompatible. For the significance of a door consists in its position within an impermeable wall, and the logic and

⁴⁹ Saqqara, Q.S. 2105.

⁵⁰ Reisner, *Tomb Development*, pp. 238-43.

⁵¹ There is no need to refer to the problem of the square stone showing the dead at table and which forms part of the "false door" in most Old Kingdom tombs, except for the fact that it is often treated together with the history of tomb architecture. Scharff, for instance, makes here again a distinction between Upper and Lower Egypt, while Reisner connects his "slab-stela" or "niche stone" with the Abydos stelae (both in *Studies Presented to F. Ll. Griffith*). See also H. W. Müller, in *MDIAA*, IV, 165 ff. and Junker in *Giza I*, pp. 13 f. and *Giza II*, pp. 14 f. *Annales du service*, Vol. XIII, Pl. IV shows how at Giza too the combination of the funerary chapel (made of bricks) and the mastaba produced the effect of a "false door."

⁵² Reisner, *Tomb Development*, pp. 282-87.

⁵³ Petrie, *Denderah*, Pl. 29.

clearness of the First Dynasty solution where two slits were made near to each other in a smooth wall (Fig. 7) and of the usual Egyptian usage of showing one or two "false doors" in an otherwise unbroken surface naturally ousted the recessing from tomb architecture.

It is significant that the one application of recessing which found a place in the normal Egyptian tomb served to isolate the "false door" and to enhance its effect. This use of recessing is found in the "Ka-door" in its elaborate "palace façade" form; it consists of a narrow slit, the actual door, flanked on either side by a standardized set of recesses, the whole cut into a monolithic slab of stone (Pl. I, D). A new, composite unit had been created, and the recesses were no longer by themselves the elements of decoration.⁵⁴ Thus the recessed building suddenly introduced under the first kings of the First Dynasty leaves its trace in the form of the "false door." To start from the latter, though it is better known, and to attempt to explain the First Dynasty building in that way, means putting the cart before the horse.

III

We have found the current explanations of the recessed architecture of the First Dynasty inadequate, even irrespective of the fact that they fail to account for the contemporary construction of similar buildings in Mesopotamia. If, on the other hand, we assume that the Egyptians of the First Dynasty derived from Mesopotamia the peculiar form of brick building which is the first to appear in Egypt, all facts fall quite simply into place and become understandable. Now this assumption does not need to be made *ad hoc*. There is a considerable number of phenomena which require for their explanation a somewhat sustained and elaborate acquaintance, on the part of the Egyptians, with Mesopotamian culture. Several of these have long been known; others have recently been placed in their true significance by discoveries in Mesopotamia.⁵⁵ We here enumerate them once more

⁵⁴ This new unit is, in its turn, used decoratively in some sarcophagi like that of Mycerinus, which are decorated all around with "false doors" of the elaborate type. Note that in contrast with the recesses in Hesire's tomb the decoration of these coffins had no ritual function, since the living never approached them.

⁵⁵ See Scharff in *ÄZ*, LXXI (1935), 89 ff. His synchronistic table can be simplified in the manner indicated in our text, and his interpretation of the evidence from the seals seems incorrect but can now be dispensed with in any case. It is unfortunate that the relief of shell which he discussed in a separate article (*MDIAA*, VI, 103 ff.) must be ex-

in a somewhat systematic form because their implications are clearer when it is realized that they do not represent a random collection of resemblances:

MESOPOTAMIAN INFLUENCE IN PRE- AND PROTODYNASTIC EGYPT

I. EVIDENCE OUTSIDE THE FIELD OF ART

- a) Mesopotamian objects found in Egypt:
 1. *Three cylinder seals* of the Jemdet Nasr period
- b) Mesopotamian usages temporarily adopted in Egypt:
 1. *Sealing* with engraved cylinders
 2. *Recessed brick-building* for monumental purposes
- c) Mesopotamian objects depicted on Egyptian monuments:
 1. *Costume* on the Gebel el Arak knife handle
 2. *Scalloped battle-ax* on fragment of late predynastic stone vase
 3. *Ships*, on Gebel el Arak knife handle, "decorated" vases, stone vase, and ivory labels of First Dynasty

II. EVIDENCE IN THE FIELD OF ART

- a) Mesopotamian motives depicted in Egypt:
 1. *Composite animals*, especially winged griffins and serpent-necked felines, on palettes and knife handles
 2. *Group of hero dominating two lions*, on Gebel el Arak knife handle and in tomb at Hierakonpolis
 3. *Pairs of entwined animals*, on knife handles and Narmer palette
- b) Mesopotamian peculiarities of style apparent in Egypt:
 1. *Antithetical group*, on knife handles and palettes
 2. *Group of carnivore attacking impassive prey*, on knife handles
 3. *Drawing of musculature*, on Gebel el Arak knife handle

In comment something has first to be said about the time at which the intercourse between the two countries took place. The imported Mesopotamian cylinder seals obtained in Egypt are particularly valuable in this respect⁵⁶ since they are typical for the Jemdet Nasr period and unknown in the preceding Uruk period of Mesopotamia. And

cluded altogether. It is certainly the copy in hard material of a tablet of the Jemdet Nasr period with two seal impressions and some numerals. But its connection with Egypt consists merely in the hieroglyph *mr* which Scharff saw in the engraved sign on one face. Closer scrutiny, however, reveals that it is no such thing, but an ordinary picture of the Mesopotamian plow! If the object is turned on its side with the left side as base, we clearly see on the right of the engraving the small crossbar at the end of the pole, below it the colter, and, on the left, somewhat more vaguely, the two handlebars and the seed funnel. Compare the clear renderings in, e.g., my *Cylinder Seals*, Pls. XXI_f; XX_d; XXI_e.

⁵⁶ Frankfort, *op. cit.*, p. 293.

this useful distinction cannot be made in respect to most of the other features listed here which, as far as we know at present, may occur in either period.⁵⁷ We have, however, already seen that the presence of towers flanking the entrance is similarly unknown before the Jemdet Nasr period.

Moreover, there is entirely independent evidence to prove the synchronism. This derives from the work of the Syrian Expedition of the Oriental Institute and more especially from the stratigraphical work carried out by Mr. Robert Braidwood, to whom I am much indebted for permission to use the information. In certain layers at Tell Judeideh, seals of the Jemdet Nasr period were discovered, as well as pottery of the type which was found as importations in the tombs of certain First Dynasty kings in Egypt and notably of Zet. A great deal of corroborative evidence, involving Palestine also, cannot, of course, be discussed here, but the fact that the Jemdet Nasr period is contemporaneous with the Late Predynastic period and the early part of the First Dynasty must be considered proved.

It should be remembered that this conclusion is quite independent from the material which is the subject of this paper. It should also be remembered that Mesopotamian influence upon Egypt would be easier to explain in the Jemdet Nasr period than at any other time. For this was an age of great Mesopotamian expansion; its tablets and seals have been found not only in Elam but on the central Iranian plateau as well, at Siyalk near Kashan; its cylinder seals occur as far afield as North Syria, Anatolia (Alishar and Troy), and even in the Cyclades.⁵⁸ Thus their occurrence in Egypt, and in fact the very varied signs of Mesopotamian influence at this time, harmonize closely with the known trends of the period.

The evidence in our table which does not concern art is exceptionally important because stylistic testimony is often subject to suspicion. It might be argued, for instance, that the antithetical group was so well in keeping with the general tendency toward a stricter order in politics, art, and religion that its temporary adoption needed no fur-

⁵⁷ The group of the hero between animals is not known on cylinder seals before the Second Early Dynastic period but occurs in exactly the same form as on the later seals on a sculptured stone vase of the Jemdet Nasr period found by us at Tell Agrab (*Illustrated London News*, September 12, 1936, p. 134, Fig. 16).

⁵⁸ Frankfort, *op. cit.*, pp. 227 ff.

ther explanation. This point of view cannot account, however, for the appearance of a hero in purely Mesopotamian costume on the Gebel el Arak knife handle⁵⁹ nor for the scalloped ax which appears on a sherd of a late predynastic stone vase⁶⁰ but which was only adopted in Egypt by the end of the Old Kingdom although it was known in Mesopotamia from the beginning of the Early Dynastic period, and possibly used earlier; nor, as we have seen, for the sudden appearance of a sophisticated form of architecture not used in Egypt on any scale before this age, while it was indigenous from very early times in Mesopotamia.

But even the purely stylistic resemblances are in their totality more significant than a mere accidental group of resemblances. If we exclude the rendering of musculature which distinguishes the hero on the Gebel el Arak handle from the undifferentiated limbs of the usual predynastic figures, we notice that the features which we claim to be derivative in Egypt not only resemble individual Mesopotamian designs but possess one feature in common which is as characteristic for Mesopotamia as it is alien to Egypt: they are all pronouncedly unrealistic. By their predominance of imagination or design over probability or nature, they illustrate the remarkable power of abstraction of the Mesopotamians. The same mentality made the pictograms which underlie Mesopotamian writing lose almost at once all resemblance to the objects which they depicted, to end in the entirely abstract wedge groups of cuneiform writing. Egypt, starting also from pictograms, retained throughout the clear pictures which we call hieroglyphs and even exploited their representational character in later times by puns or "enigmatic writing."⁶¹

The disappearance of the features enumerated in our table becomes understandable also if we remember that they are derivations. Already in the Third Dynasty, brick gives way to stonework for monumental funerary structures. The seal cylinder is replaced by the scarab for all practical purposes after the Old Kingdom. Of the other features in our list, not one survives the early reigns of the First Dynasty.

⁵⁹ For this monument, and all others referred to, see Scharff's article quoted in n. 55.

⁶⁰ Scharff, *Mitteilungen aus der aeg. Sammlung*, Vol. V, Pl. 22, No. 108.

⁶¹ Enigmatic script: E. Drioton, "La Cryptographie égyptienne," *Revue Lorraine d'anthropologie*, Vol. VI; puns: e.g., Griffith, *Beni-Hassan*, Vol. III, Pl. VI, 82.

Contact with Mesopotamia may have stopped then, and in any case the vitality of Egypt was such that it would certainly not retain unassimilated foreign matter. It would be absurd to consider Egyptian civilization in any way derivative. There is no necessity to assume Mesopotamian influence in order to explain the development of pharaonic civilization, but it so happens that we have evidence that such influence was, in fact, exercised. Our view is not, therefore, a theoretical postulate but a conclusion based on observed facts which seem to suggest it.

This influence from the east may well have worked merely as a catalyst. It is curious that there is no evidence of contact on the Mesopotamian side. Since it was exercised, apparently, via the Red Sea route and affected Egypt most noticeably in the region around the terminus of the Wadi Hammamat, it is possible that some thoroughly "Mesopotamian" culture on the Persian Gulf or Arabian coasts was the center involved on the Asiatic side and that such signs of intercourse with Egypt as might be preserved there never reached Mesopotamia proper and have also evaded the modern excavator. However that may be, there can be no doubt that intercourse did take place and that it greatly stimulated Egypt during the formative phases of its own culture.

ORIENTAL INSTITUTE
UNIVERSITY OF CHICAGO

THE OLD ARAMAIC ALPHABET AT TELL HALAF

THE DATE OF THE "ALTAR" INSCRIPTION

RAYMOND A. BOWMAN

In 1931 Baron von Oppenheim, reporting on his excavation at Tell Halaf, wrote: "In der Kaparaschicht fanden wir ein kleines Kalkstein-altärchen mit einigen wenigen, leider nicht entzifferbaren altsemitischen Schriftzeichen."¹ No further stratigraphic detail has been released on this piece which, therefore, must be discussed apart from any exact archeological context. Meissner, from the character of the cuneiform signs, peculiarities of language, and the point of view expressed in the short cuneiform inscriptions of Kapara, feels that a date in the twelfth century B.C. is fitting for the Kapara stratum.² Von Oppenheim insists that the date of the small finds of the stratum and the views of B. Landsberger with regard to the earliest possible presence of Aramaeans in Mesopotamia also support the twelfth century B.C. date.³ The twelfth-century date has been widely accepted⁴ but not without considerable opposition.⁵

The publication of the Kapara Aramaic inscription, long awaited, has finally appeared as the work of Johannes Friedrich.⁶ A hand copy of the inscription is given (Fig. 1) but, unfortunately, no photograph. Friedrich transliterates it as כרר | זי | [[x]] בעמ | זדמת but, with re-

¹ M. F. von Oppenheim, *Der Tell Halaf* (Leipzig, 1931), p. 65; cf. English translation by G. Wheeler, p. 70. The expanded French edition, which I understand is in preparation, is not available.

² B. Meissner, *apud* von Oppenheim, *op. cit.*, p. 266 (English ed., p. 316). S. Langdon, *Oxford Magazine*, June 15, 1933, p. 812, on the basis of the cuneiform signs, dated the Kapara inscriptions to the tenth century B.C. There can be no certainty in dating based on cuneiform script alone.

³ Von Oppenheim, *op. cit.*, p. 64 (English ed., p. 68).

⁴ Cf. W. F. Albright, "The Present State of Syro-Palestinian Archaeology," in *The Haverford Symposium on Archaeology and the Bible* (New Haven, 1938), p. 27. Albright, however, modifies the date to "twelfth or eleventh century."

⁵ For a summary and bibliography of some of the outstanding opponents of the Meissner-von Oppenheim date see E. Herzfeld, "Der Tell Halaf und das Problem der hettischen Kunst," *Archaeologischen Mitteilungen aus Iran*, VI, Part 3/4 (1934), 112 ff.

⁶ J. Friedrich, A. Ungnad, G. R. Meyer, E. F. Weidner, *Die Inschriften vom Tell Halaf* (Beiheft zum *Archiv für Orientforschung*, Vol. VI [Berlin, 1940]), pp. 68-70 and Pl. 29.

gard to translation, says: "Die kürze des Textes und einige Beschädigungen erschweren die Lesung, eine Übersetzung der paar Zusammenhängenden Zeichengruppen ist leider nicht möglich."

Concerned about the distribution of letters in the first group, he suggests that the first letter might be the demonstrative pronoun and the remainder the Akkadian *dimtu*, "pillar"; but he is troubled by disagreement in gender. For the second group of letters he has no suggestion but remarks that, after the *mem*, "Dann beginnt die Zerstörung um die Kante herum, in der etwa ein bis zwei Zeichen Raum hätten, doch ist möglicherweise auch diese Kante ebenso schriftfrei gelassen worden wie die folgende." His copy, however, shows no such extensive damage. He rightly reads the next letters as the Aramaic relative pronoun, which classifies the language of the inscription. The final

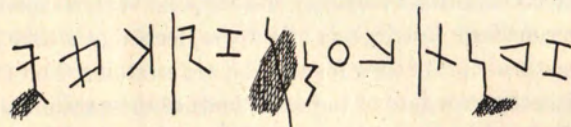


FIG. 1

group he reads as two words and translates "denn lebend" or "denn er lebte."

The decidedly anachronistic forms of some of the letters disturb Friedrich. His *beth* he recognizes as much too late a form for the *delta* and *mem*, which suggest the old Phoenician characters from Byblos. He was tempted to read an archaic *aleph* retrograde or possibly a *kaph*, such as occurs later in the inscription, but finally abandoned both as less likely than an open-headed *beth*. The next to last letter he regards as an unusual *heth* bearing only two bars. He should rightly be concerned over such troublesome letters, for they certainly suggest that the relationships with the Byblian script, proposed for the remainder of the inscription as supporting the doubtful date of about 1200-1100 B.C. for Kapara, must be called into question.

The results I achieved when I copied the same inscription in the Tell Halaf Museum, through the kind permission of its owner, were quite different (Fig. 2) and would lead to another conclusion with regard to its date. My transliteration reads⁷ . . . כתר | די | כתר | זבית.

⁷ The reading *beth* is discussed below.

It will be noted that in my copy those anachronistic letters which seemed to link this piece with the earlier inscriptions from Byblos disappear completely. Zigzag lines, which would produce the "Byblos" *mem*, are, as epigraphers know, perhaps the most common of copyist errors.

There remains doubtful, however, the second letter, which is incomplete. The form at once suggests that it should be completed as a *delta*-form *daleth*. Such a letter, when compared with the others, would be slightly anachronistic, although not absolutely impossible. It is significant for dating that such a *daleth* is found in Phoenician inscriptions as late as the eighth century B.C. along with forms showing an incipient staff. Such letters are found on the bronze fragments of the Ba'al Lebanon bowl from Cyprus which mentions Hiram (II) of Tyre.⁸ By

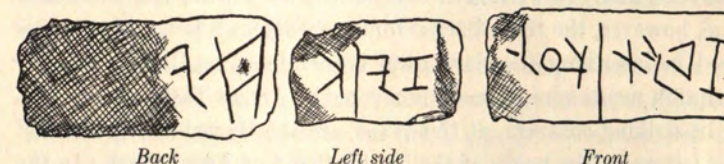


FIG. 2

this period the earliest Syrian inscriptions from Zenjirli, written in a much more cursive style, have forms with a very short staff.

The defective letter might be completed in another fashion, however. Only a short horizontal stroke, easily omitted by a careless writer, needs to be added to convert the letter into a *beth* that would be consistent with all the other letters of the inscription. This single stroke would at the same time convert an awkward combination of letters into an intelligible and plausible word. It is highly probable that the letter is to be read as *beth* rather than as *daleth*.

More damaging for a theory of an early date for this writing is the form of the *kaph*. The letter appears in the early Byblos script as a fan-shaped triad of equal lines joined at a point below. This form oc-

⁸ CIS, I, 5, Pl. IV, Frags. A, D, and F of the best-preserved bowl. This inscription has been dated by Lidzbarski to a period close to the second millennium B.C. (*Handbuch der Nordsemitischen Epigraphik* [Weimar, 1898], p. 176). Z. Harris, *A Grammar of the Phoenician Language* (New Haven, 1936), p. 157, dates it with some question to the ninth century B.C.; while A. T. Olmstead, *History of Palestine and Syria*, p. 434, and *History of Assyria*, p. 183, places the bowl in the time of Tiglath-pileser III.

curs regularly as late as the Eli-Ba'al inscription (925-889 B.C.) from Phoenicia⁹ and would be expected in the Kapara inscription if it showed affinity with the early alphabet. The Kapara forms most closely resemble the early Greek *kappa*'s from the island of Thera, dated to the eighth and seventh centuries B.C.¹⁰ Similar forms, but with longer staffs, are found on the Lebanon bowl mentioned above. The cursive *kaph*'s of the Mēsha' and Kilamū inscriptions are morphologically a bit later in their development than that of the Kapara legend, since the trend appears to be in the direction of a longer staff.

Most critical for dating, too, is the *heth* with two bars¹¹ which Friedrich recognizes as having no parallel in the early Byblos inscriptions, where a three-barred variety prevails.¹² In Moab the two-barred form is found as early as the last half of the ninth century B.C. and is regularly so written in the Moabite stone. In Syria and Palestine, however, the three-barred form persists for a longer time. It is used in inscribing the Samaritan ivories¹³ and on the Aramaic inscription mentioning Hazael discovered at Arslan Tash.¹⁴

In striking contrast, at this point, are the Hazael inscription and the letters on the backs of the ivories found at Arslan Tash. In the small group of the latter there are three *heth*'s of the two-barred variety to one with the three bars.¹⁵ If all these ivories are assumed to belong to the same piece of furniture, they would form a transitional stage in writing such as is represented by the Samaritan ostraca where, however, the three-barred form is still the most frequent.¹⁶ The recent tendency to date the ostraca to the eighth century B.C.,¹⁷ rather than

⁹ R. Dussaud, *Syria*, VI (1925), Pl. XXV and copy on p. 109.

¹⁰ H. Roehl, *Imag. Inscr. Graec. Anti.* (3d ed., 1907), I, No. 6, 2.

¹¹ In my copy the mark within the letter indicates a rather deep hole.

¹² There is the possibility of a single two-barred form in the Yehimilk inscription from Byblos of the twelfth or eleventh century B.C. (cf. M. Dunand, *Revue biblique*, Vol. XXXIX [1930], Pl. XV, l. 1), but in the light of the preponderance of evidence, even within the inscription itself, this isolated form must be considered an error.

¹³ E. L. Sukenik, "Notes on Hebrew Letters on the Ivories," *apud* J. W. and G. M. Crowfoot, *Early Ivories from Samaria* (London, 1938), p. 7.

¹⁴ F. Thureau-Dangin, A. Barrois, G. Dossin, and M. Dunand, *Arslan Tash* (Paris, 1931), p. 135, Fig. 49 and Pl. XLVII, No. 12.

¹⁵ *Ibid.*, p. 91, Fig. 33, Nos. 2, 26, 27. Only No. 5 has three bars.

¹⁶ G. A. Reisner, *Israelite Ostraca from Samaria* ("Harvard University Palestinian Expedition"), Pl. VI, Nos. 22 and 24; Pl. VII, No. 26; Pl. IX, No. 31.

¹⁷ Cf. R. Dussaud, *Syria*, XVI (1935), 211; H. L. Ginsberg, *Archiv orientální*, VIII (1936), 145; W. F. Albright, *From the Stone Age to Christianity* (Baltimore, 1940), p. 314, n. 17.

to the ninth, is interesting as suggesting a probable date for the letters on the Arslan Tash ivories.

The ninth-century date for the inscribed ivories of Arslan Tash, suggested by Sukenik on the basis of their association with the Hazael inscription,¹⁸ deserves to be reconsidered. Not all the ivories were part of the same piece of furniture, datable by the Hazael name. Only one of the inscribed ivories, and that without a *heth*, is definitely mentioned by the excavators as being found in proximity to the Hazael inscription and the remains of the couch dated by it. There is a distinct difference between the forms of the *heth*'s of the inscription and those of the letters on the backs of some of the ivories. The latter seem to be later than those on the Hazael ivory. The furniture from which the inscribed ivories came may have been made later than the days of Hazael and may have been brought to Arslan Tash, then the royal city Hadatu, after the fall of Damascus in 732 B.C.

The suggestion that the Arslan Tash ivories may have come from the west, possibly from Cyprus, recalls the fact that the earliest evidence for the two-barred *heth* in the Greek inscriptions comes from the texts written in Eleutherna, Crete, in the seventh century B.C.¹⁹

In Syria proper, at Zenjirli, the three-barred *heth* persisted until the time of Bar Reku (ca. 725 B.C.), in whose inscriptions the double-barred type is the rule.²⁰ The definite two-barred form in the Kapara inscription, together with the other evidence mentioned above, definitely eliminates the Kapara letters from being considered as contemporary in date with the early Byblos script.

What, then, is the date of the Kapara inscription? As indicated above, the letters critical for dating are the *kaph*, *heth*, and possibly the second letter, even if it be completed as a *daleth*. Comparison with the letters on the Moabite stone suggests a date in the last half of the ninth century B.C. If, however, the geographically more remote Moabite letters be discarded in favor of Syrian and Palestinian letters as criteria for dating the Mesopotamian forms, a definite lag in the development of some forms of the letters must be considered, particular-

¹⁸ Sukenik, *op. cit.*, p. 8.

¹⁹ F. Halbherr and D. Comparetti, *Mus. ital. ant. class.*, II (1888), 162-63, Nos. 1a, N2 and N3.

²⁰ F. von Luschan, *Ausgrabungen in Sendschirli*, IV (1911), 379, Fig. 275.

ly with that of the *beth*. Under such circumstances comparison would be with materials of the eighth century B.C., some of which are even as late as the time of Tiglath-pileser III. If the second letter be completed as a *beth*, the argument for the later date would be strengthened. It would be safe, therefore, to date the Kapara inscription to the last half of the ninth or to the beginning of the eighth century B.C.

Mr. Braidwood, of the Oriental Institute, whom I have consulted on the possible date of the Kapara material in the light of Syrian stratigraphy, writes:

In view of the fact that the great bulk of late material excavated by the Oriental Institute's Syrian Expedition in the Plain of Antioch is still not ready for publication, I venture to append this note to Dr. Bowman's argument. Unfortunately, the reader has no choice but to accept my judgment in comparative archeology in this matter until the Syrian Expedition publication appears. It is, however, possible to say quite categorically—as will be evident when this material is published—that the bulk of the Tell Halaf small finds, published as of the "Kapara layers" or so noted in the Tell Halaf Museum in Berlin, correspond exactly with the material of Amouq phase O (= Judaidah IV), and especially with the later aspects of the phase known best on Tell Tayinat (*AJA*, XLI [1937], 8-16; *OIP*, XLVIII, 6-7). The comparisons involved are matters of detailed similarities, not merely superficial likenesses. The following list of comparisons which are to my judgment valid, present on Tell Halaf ("Kapara layers") and our sites, must suffice for the moment.

1. Architecture: "palaces" of the normal "Hilani" plan, embellished with architectural sculpture. Wooden timbers used to bond thick mud-brick walls (cf. *Tell Halaf*, p. 80; p. 82 for plans). Similar gate constructions. A great terrace-like mass of mud bricks, appended to the second phase of the "Hilani."

2. Graves and burial customs: General use of cremation with ashes put in pots along with gifts (by inference at our sites; no burials at all were found, but cf. Hama E-F and Carchemish Iron Age graves). Later in the period inhumation practiced, at least in part.

3. Ceramics: A red-slipped and burnished series in which appear tripod bowls, the mesomphalic phiale form, the horizontal bar handle, characteristic rim profiles, some incised decoration, and a black-ware variant. Painted wares of Cypriote Iron Age type. Painted wares in which the sub-Mycenean motifs are localized (cf. Hama F and Carchemish Iron Age). Simple wares of "Assyrian" type, some very fine and with intentional depressions in the surface, or decorated with "jewelry" impressions. Assyrian glazed wares in two or three colors. A geometric petal motif. A frit bowl of gadrooned phiale

form (cf. Tayinat in red-burnished pottery, Hama E and Carchemish graves, in bronze). "Zigattu" decorative knobs (cf. *Tell Halaf*, Pl. LV and Fig. 4, p. 310).

4. Metals: The first general use of iron in Syria. Bronze bowls, some fluted (gadroned phiale?), small cups which flare slightly at the lip. Fibulae. Hollow gold earrings with "knobs." Stone-set rings with granulated setting borders (cf. *Tell Halaf*, Pl. LVII, Pl. III [color]).

5. Stone objects: Basalt bowls with tripod legs and fluted sides. Tall "censer" shapes. Duck weights. Rectangular bowls with animal heads (cf. Khorsabad; *Tell Halaf*, Pl. XLIX).

6. Bone: Decorated cosmetic box lids. Boxes with five compartments (in steatite at Tayinat) (cf. *Tell Halaf*, Fig. 21b, p. 221).

Certain points pertinent to the chronology should be noted. Baron von Oppenheim and his colleagues have not yet offered their definitive publication, but in *Tell Halaf* they refer all the material listed above to the "Kapara layers," sometimes qualifying the latter by adding "and their Assyrian successors." They go beyond this generalization in the matter of the graves, which they explicitly state were used by Kapara and collateral personalities. It is therefore important to note that a good proportion of the items from *Tell Halaf* listed above come from the graves. Two shaft graves were actually sealed by a great mass of mud bricks southeast of the Halaf "Hilani" or "Temple-Palace." Since both of the latter structures also are called "Kapara dynasty" in date, we may be quite certain that—in the minds of the excavators—the tomb material is of Kapara date with no admixture from the "Assyrian" or "Guzana" layers. Schmidt (*Tell Halaf*, p. 311), who is quite specific about the ceramic content of the tombs, describes essentially what is listed above.

There is excellent evidence that the material from Tayinat listed above covers the range between 850 and 600 B.C. It is certainly from the middle-later part of our phase O (= Judaidah IV). The material of the earlier part of that phase is distinguishable from it on clear typological and stratigraphic grounds, and it is absolutely nonexistent in the previous phase N (= Judaidah V), dated by us to between 1200 and 1000 B.C. on the basis of a splendid series of "Sub-Mycenean" pottery. The Tayinat material of mid-late phase O comes from a time when that town came under Assyrian influence and even occupation, and the Assyrian objects found are in no way disturbing.

It is, of course, possible that the complex of objects listed above could be slightly earlier in the Syrian hinterland than in the Plain of Antioch, where the period from 1200 to 1000 B.C. was so thoroughly taken up by the "Sub-Mycenean" peoples. A case in point would be Hama F, which shows painted pottery (with motifs copied from the "Sub-Mycenean" tradition) and the practice of cremation (heretofore strange in Syria and certainly another "Sub-Mycenean" trait) appearing together with the earliest red-burnished

wares which could not be from before 1000 to 950 B.C. in the Amouq plain. A similar material culture of mixed traditions seems to have been present at Tell Halaf. The more advanced forms, which are listed above and check in such detail with those of Tayinat (and, evidently, with Hama E), could not, in my opinion, be before 900 B.C. at the earliest. I strongly contend that the published material of the "Kapara layers" at Tell Halaf, sculpture and all, is almost directly contemporary to the period of large public buildings on Tell Tayinat, and that, like Tayinat, there is no gap whatsoever between it and the appearance of Assyrian material which is dated at Tayinat by an inscription of Tiglath-pileser III.²¹ Assyrian material is simply added to the sum total of the culture. My own opinions aside, this note needs do little more than remind the reader that Dr. Bowman's evidence will acquire impressive support when the later Plain of Antioch volumes appear.

Thus, by independent evidence, epigraphic and archeological, the Kapara stratum at Tell Halaf can be checked and dated.

The inscription on the *altärchen* is not illegible. There is little doubt about the majority of the letters. But it is difficult to be certain regarding the meaning of the combinations of letters. The initial letter is doubtless a prefixed demonstrative pronoun, but the group that it precedes is baffling if its first letter is to be read as *daleth*. The demonstrative suggests that the name of the object should follow. Unfortunately, the top of the object is missing, and there seem to be no parallels usable for its identification. That it is a miniature altar is possible. It may be a small household *maššebah*.²² Such an object might be described as the "abode" of the god,²³ a reading that would result from calling the incomplete letter of the difficult group a *beth*. The next word would then be, by context, some divine name for which I can offer no parallel or identification. Then follows the one certain group, the Aramaic relative pronoun.

More difficulty is encountered with the final group, which rounds the corner. Should the letters be regarded as forming a single word?

²¹ Dr. Calvin McEwan calls attention to the fact that an inscribed fragment from the stratum at Tayinat bears the name of Kalparunda of Hattina, a contemporary of Shalmaneser III (858-824 B.C.). This find (cf. I. J. Gelb, *Hittite Hieroglyphic Monuments* [OIP, Vol. XLV], p. 39, No. 52) would push back the possible date of the stratum to a point where it would be in complete agreement with the earliest possible date for the Aramaic inscription under discussion.

²² A similar but much larger altar, also inscribed on its base, probably contemporary with the Tell Halaf piece, was found *in situ* just outside of a cult building on Tell Tayinat (cf. I. J. Gelb, *op. cit.*, Pl. LXXXIV, Fig. 53).

²³ Gen. 28:22.

If so, one at once thinks of the Hebrew *kōhî*, "my strength," which would produce an intelligible and appropriate, if somewhat exalted, sense, "This (is) the abode of . . . who (is) my strength." But *kōhî* is not found elsewhere in Aramaic except in the later Jewish Aramaic, where it doubtless occurs under Hebrew influence. If the meaning can be derived from the Ethiopic word "rock" (*kuakueh* < **kuahkueh*), sometimes cited as a cognate to the foregoing Hebrew word,²⁴ another fitting translation will ensue, "This (is) the abode of . . . who (is) my rock."

But the problem of translation is not so easily solved. If the *kaph* be regarded as a separate unit, as its presence on the side surface rather than on the back with the following letters might suggest, there would be scarcely another alternative to Friedrich's translation of the group as mentioned above. The matter is complicated by the fact, not indicated in the published copy but clearly suggested by my own, that there seems to be extensive damage to the back of the stone immediately after the last legible letter. We may have to translate, "This (is) the abode of *K^cI* who is like *HI*. . . ." One may have to reconstruct the last word, if it is incomplete, as a verb, but the radicals suggest nothing appropriate; one can only conjecture what it might be.

It is unfortunate that the meaning cannot be easily and unquestionably recovered. But translation in this case is not the most important factor. It is in the interest of establishing the text and its probable date that this is written. The piece is important beyond its mere text. Let us hope that the future will see both the significance of this inscription and the role of Kapara in the later Assyrian empire clarified.

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²⁴ Cf. Ges.-Buhl, *Handwörterbuch* . . . ¹⁴, p. 306.

THE STORY OF JERICHO

FURTHER LIGHT ON THE BIBLICAL NARRATIVE

JOHN GARSTANG

Revision in the dating of certain types of pottery from Beisan,¹ as proposed by Dr. Albright and accepted in principle by Rowe and FitzGerald, the excavators of that site, calls for a correlation of results elsewhere, including Jericho. Here it involves in chief a class of painted wares² decorated in monochrome or shaded duochrome with linear or triangular devices, which was formerly assigned to the first half of the Late Bronze Age but is now ascribed to the second half of that period, from the time of the pharaoh Seti I (ca. 1320 B.C.) into the next century.

Personally, I have always found it difficult to class these painted wares, and the common pottery associated with them, in the same culture phase as those which belong to the age of Thutmose III, as seen, for example, at Megiddo.³ The painted lines seem relatively thin and spidery and the original motives are tending to be lost: the snake, for instance, is often rendered quite conventionally by a wavy line. The tones used are more frequently only shades of a single color rather than contrasting and distinct; while the pottery itself, though for the most part well made, is thinner on the whole and of a paler shade and was made apparently on a faster wheel. The common pottery shows also noticeable variations: the tapering vessels are fatter and less elegant than of old, while the lamps show pronounced tendency toward reinforcement of the rim. I was also perplexed by the fact that none of these painted wares appeared in the tombs; and I did in fact class them at first as LB II; but the evidence from Beisan then generally accepted made it necessary to find a place for this group before the close of LB I. My misgivings were revived in 1939 by a further study

¹ Albright, *AASOR*, XVII, 86; FitzGerald, *PEFQS*, 1940, p. 81; Rowe, *Four Canaanite Temples*, I, ix.

² *PEFQS*, April, 1935, Pl. I and p. 68; *AAA*, Vol. XXI, Pls. XXX-XXXIX.

³ Engberg and Shipton, *SAOC*, No. 10, in Strata VIII and IX.

of the Jericho materials with a view to a final publication of our results, hitherto confined to progress-reports in the University of Liverpool's *Annals of Archaeology and Anthropology*. With the specimens in question thus clearly in mind, though without access to my notes and records at the time of writing, I have no hesitation in accepting the proposed revision in principle, and like other students I shall be glad to see an agreed reclassification made in due time by the excavators themselves together with the publication of their very interesting fifteenth-century pottery, which is as yet known only to a few privileged friends.

The later painted fabrics were found by us at Jericho in considerable quantity upon the small mound overlooking the spring during the excavation of the Middle Building, a structure so named because it stood in apparent isolation, stratified between the so-called Hilani above and the older palace storerooms below.⁴ In previous excavations Dr. Watzinger had also found specimens on the same spot. His last-minute excavation of the Hilani had penetrated to its foundations, which were exceptionally deep in proportion to its massive character, and so disclosed some underlying walls of the Middle Building, as shown in his published plan,⁵ and with them some of its pottery. His personal records, which he kindly communicated to me in reply to my inquiries, left no doubt that he had reached in places and probed the upper levels also of the palace storerooms. Thus is explained how fragments of the older painted fabrics came to be found in the debris of the Middle Building and how a number of these fitted together, though found by us in different rooms or areas.⁶ Notwithstanding the evident disturbance of the strata, the original stratification of the pottery under discussion is clarified by the present recognition of its later and earlier elements. The later series clearly belongs to the Middle Building in which it was found exclusively; and this was confirmed by my recent re-examination of the materials which include a number of tell-tale pieces. The earlier series must perforce be assigned to the underlying stratum, and this also seems to be confirmed by the discovery of two whole vessels of this class in the topmost levels of the palace storerooms—in spots, it should be noted, that lay outside the

⁴ *The Story of Jericho*, Figs. 3 and 4.

⁵ *Jericho*, Table IV.

⁶ *AAA*, Vol. XXI, Pl. XXXIX, and p. 103.

area of the Middle Building and were accordingly freer from disturbance.⁷ These vessels are similar to those from Tomb 5, where they seem to date their incidence to the age of Thuthmes III. Another type of painted wares—the last of our LB I series—is represented by a number of long-necked jugs found in Tomb 4; they appear to imitate in a way the larger type of Cypriote jugs of *bil-bil* fabric; and their introduction as a type into the repertory of Bronze Age Canaan seems to be dated both at Jericho and at Lachish⁸ by contemporary scarabs of Amenhetep III.

A few specimens of the later class of painted wares that we have been discussing were also found, by both myself and Dr. Watzinger, at the north end of the site, outside the limits of the Fourth City. Ours came from a group of rooms against the Hyksos defensive parapet, and it was with interest I noticed that I had in fact dated them at the time to LB II.⁹ So far as I am aware, none of this distinctive pottery was found by either expedition elsewhere on this site. Certainly, none was found in association with the main walls of City IV, and, as already stated, none was found in any of the tombs. The evidence for dating the occupation and destruction of the Fourth City remains thus unchanged by this revision: the Middle Building is simply dissociated from the period when that city was in being.

Though as a result of this revision the Middle Building cannot have been the residence of the last kings of Jericho (as the indications and criteria led us to suppose at the time of its discovery)¹⁰ a reinterpretation of the evidence in the light now available discloses a possibility of high interest. Our excavations, logically interpreted, point to the fall of the city in the reign of Amenhetep III (ca. 1400 B.C.), possibly late in his reign (which is well represented), but before that of his successor Akhenaton, of whose period there is no trace—no royal signet, no influx of Early Mycenaean pottery, and no mention of Jericho in the

⁷ Cf. report for 1933 in *ibid.*, Pl. XXIV, No. 6, found with a Cypriote milk bowl in the top layer of house-room No. 60; and Pl. XXVII, 15, from the uppermost layer of spot 17a, in square I.6; also cf. Pl. XX, No. 1, from Room 39.

⁸ Jericho: AAA, Vol. XX, Pl. XI, and p. 26; Lachish: *The Fosse Temple*, Pl. LI.B.ii.

⁹ *PEFQS*, 1930, pp. 130 ff., and *Joshua and Judges*, pp. 146–47.

¹⁰ The caption to the photograph of the Middle Building in *The Story of Jericho*, Pl. XVIa, needs to be corrected in the light of this revision. So also does the opening paragraph on p. 118; the fragments of painted pottery like that from Tomb 5 belong in all probability, as we have seen, to the underlying stratum (cf. AAA, Vol. XX, Pl. XXV).

Amarna letters. But we have recognized traces of a partial and intermittent occupation of the site, with a few intrusive burials in the tombs,¹¹ during the five hundred years that the city itself lay in ruins. To these traces must now be added the Middle Building and its contents.

Looking at the plan of this structure, we find certain curious features: though clearly a residence (for it had both hearth and oven), one long room in the main block was like a stable; and it was also provided with its own stout inclosing wall which was laid out noticeably askew from the old lines of the city. What can this strange intrusion signify? To what alien occupier can it be attributed, who secured for his dwelling the most favored position on the site, but who apparently made no use of the tombs? The Bible itself provides the answer. In Judg. 3:12–14 we read:

... The Lord strengthened Eglon the King of Moab against Israel. . . . And he gathered unto him the children of Ammon and of Amalek. And he went and smote Israel, and they possessed the City of Palm Trees. And the children of Israel served Eglon the King of Moab eighteen years.

The approximate date of this episode is not difficult to compute if we accept as basis the date of the fall of Jericho. This, according to biblical tradition, would fall about 1400 B.C. or just before, a result in very close agreement with that of our excavations which, we have seen, point to 1400 B.C. or just later. Both sources indicate indeed the same reign (that of Amenhetep III); and the central figure of 1400 is the nearest approximation to the date that evidence can support. With this as basis, the date of the oppression by Eglon, according to data given in the Bible (which include the period of Joshua and the Elders, the eight years of oppression by Cushan, followed by forty years of Rest),¹² would fall wholly or mostly within the reign of Seti I. But this is the very period to which archeologists now ascribe the incidence of

¹¹ Tombs 4 and 13. Wace has shown that the pseudo-Mycenaean vases from the latter cannot be earlier than 1350 B.C. (cf. *Annual of the British School in Athens*, 1940 [Session = 1936–37], pp. 259 ff.).

¹² The biblical traditions as to dates are discussed in *Joshua and Judges*, pp. 60–62, and a table of approximate and relative dates based on Egyptian chronology is given in *ibid.*, p. 243. The general estimate of 480 years (I Kings 6:1) from the Exodus to the fourth year of Solomon is controlled by the figure of 300 years from Heshbon to the first year of Jephthah (Judg. 11:26) and harmonizes with it if in both cases we regard the Minor Judges as contemporary with and not independent of the Major Judges—a point upon which most students are independently agreed.

the pottery we have discussed above. That this pharaoh did repress disturbances in the Jordan Valley is known independently from an inscription discovered at Beisan; and that the disturbances extended farther south is recorded in the Egyptian archives of his reign: "The vanquished Shasu [Beduin] plan rebellion: their tribal chiefs are gathered together, rising against the Asiatics of Southern Palestine [Kharu] . . . and they disobey the laws of the Palace."

It is instructive to compare closely the text of this historical record with that of the biblical episode previously quoted. The parallelism is complete. Each record tells of a combine of border tribes from the southeast and beyond Jordan menacing the southern highlands. Moreover, they agree closely in date if we accept the basis of Israelite tradition, and they may well refer to one and the same episode, to which the ruins of the Middle Building on the site of old Jericho bear material witness. The scholars who have revised the dating of Late Bronze Age pottery have also restored a fragment of Bible narrative to its rightful place in history.

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DARIUS AND HIS EGYPTIAN CAMPAIGN

RICHARD A. PARKER

The verso of the so-called "Demotic Chronicle" contains, among others, the well-known statement of the codification of Egyptian law under Darius. Following the translation of Spiegelberg, later writers have consistently given *year 3* as the one in which Darius sent to his *satrap* in Egypt, ordering the compilation of the laws.¹ Actually, the year number is *4*, as was recognized by Spiegelberg himself in his glossary, although he failed to correct the translation.² It was in 518 B.C., therefore, that Darius sent the command to his satrap.³

According to the Behistun inscription (§ 21), Egypt revolted while Darius was fighting Nebuchadnezzar III, but no campaign against it is recounted.⁴ Herodotus tells us that Aryandes, the satrap whom Cambyses had appointed, was executed for assuming the royal prerogative of coining money, though Darius charged him only with rebellion.⁵ Whether it was the coinage of money, the ill-fated expedition against Barce, or other events unknown to us, the fact that Egypt was considered a rebel, and that Aryandes was put to death as one, suggests that Darius, a young, vigorous, and energetic king, took care of Egypt at his earliest opportunity. Furthermore, one may conjecture that the use of *his* satrap in the passage referred to above means a man appointed—or, at least, confirmed in office—by Darius. This could not have been true of Aryandes.⁶ Nor does it seem likely that

¹ W. Spiegelberg, *Die sogenannte Demotische Chronik* (Leipzig, 1914), p. 31; E. Meyer, *Ägyptische Dokumente aus der Perserzeit* ("Sitz. d. König. Pr. Akad. d. Wiss. phil.-hist. Kl.," 1915), p. 308; N. J. Reich, "The Codification of the Egyptian Laws by Darius and the Origin of the 'Demotic Chronicle,'" *Misraim*, I (1933), 180; G. Posener, *La première domination Persée en Égypte* (Le Caire, 1936), p. 175.

² *Op. cit.*, p. 144. The difference between the signs for 3 and 4 is clear, though the significance of the small oblique stroke over the sign in the text is puzzling.

³ His fourth year began December 31, 519, in Egypt.

⁴ I have discussed this paper with the editor, who has given me a number of suggestions and references to Persian material.

⁵ iv. 166. No coins definitely ascribable to Aryandes have yet been found. R. W. Rogers (*A History of Ancient Persia* [New York, 1929], p. 98) revives an old error in assigning to him coins long before recognized as belonging to the kings of Sidon (cf. E. Babelon, *Traité des monnaies grecques et romaines* [Paris, 1910], II, No. 2, 546).

⁶ We know that Cambyses appointed him (Herodotus, *loc. cit.*) and that Egypt was considered in revolt before three months had elapsed after the death of Bardiya and the accession of Darius (Beh. § 21, and Cameron, "Darius and Xerxes in Babylonia," *AJSL*, LVIII [1941], 314 ff.).

Darius would have concerned himself with the codification of the laws of his empire, before that empire was completed by the reacquisition of Egypt, probably its most valuable province.⁷ It is possible to conclude, therefore, that this message was sent after Darius had taken Egypt, killed Aryandes, placed a new satrap in power, perhaps Pharandates, and returned to Asia.

With the latter part of 518 as an *ante quem* for Darius' Egyptian campaign, what can be said about a *post quem*? As the pacification of Egypt is not mentioned in the Behistun inscription, it is logical to assume that it took place after the last event recorded in that inscription. Furthermore, the march must have been through Palestine, and, as this was the period of the rebuilding of the temple in Jerusalem, we should expect to find some echo of the Persian passage in the Bible.

Behistun § 71 begins with the statement of Darius: "This is what I did . . . after I became king." The most plausible reconstruction of the broken passage is that of Tolman,⁸ who suggests: "during both the second year and the third year."⁹ Two events follow, and it seems reasonable to conclude that the revolt of the Elamite Atameta¹⁰ took place in the second year and Darius' expedition against the Saka¹¹ in

⁷ The "restoration of order" inscription of Darius, Susa e = No. 15 (cf. Kent, *JAOS*, LIV [1934], 40 ff.; *ibid.*, LVIII [1938], 112 ff.; Weissbach, *ZDMG*, XCI [1937], 80 ff.) certainly indicates law codification throughout all the empire. Its date remains uncertain. Judging from the provinces named, it must come from before the European Saka venture but after the Persians had learned of the two types of eastern Saka (cf. n. 11 below). It does not seem possible to tie Susa e in with the Egyptian order of year 4. The latter must represent only the initial step, whereas the former more probably has to do with the final promulgation of the codified laws.

⁸ *Cuneiform Supplement* (New York, 1910), pp. vi. 39. Cf. also Kent, *JAOS*, LVIII (1938), 675 f. Poebel, *AJSL*, LV (1938), 293 ff., places the Elamite revolt and the expedition against the Saka in the same year, the second. This seems possible, chronologically. Hinz, *ZDMG*, XCIII (1939), 370 ff., restores "in that fifth year," but to this there are chronological objections. By the fifth year, Egypt had been pacified, and failure to include this accomplishment in the inscription would be difficult to explain. Cf. also n. 15 below.

⁹ Darius is not reckoning here in Babylonian regnal years but means the second and third year-periods after September 29, 522 B.C., when he killed Bardiya. This is certain from his claim in §§ 52, 57, 59, and 62 that the events in the main inscription took place in one year (cf. Poebel, *AJSL*, LV [1938], 298). Actually, he did not defeat Nebuchadnezzar IV until November 27, 521. The appendix, then, covers roughly the period from November 28, 521, to September 28, 519.

¹⁰ The restoration of F. W. König, *Relief und Inschrift des Königs Darius I am Felsen von Bagistan* (Leiden, 1938), p. 78.

¹¹ These must be the *Sakā tigrakhaudā*, the "Pointed-hat" Saka, as indicated by line 22 and the sculpture of Skunkha (see also Hinz, *op. cit.*, pp. 364 ff.). In Beh. §§ 6 and 21, the land of the Saka is grouped with eastern and northeastern countries, and in Darius, Pers. e, § 2, it is placed definitely in the east. The Naqsh-e Rostam a inscription of Darius locates the *Sakā haumavargā* and the *Sakā tigrakhaudā* in the east, and the *Sakā tyaiy paradraya*,

the third. His aim would be to secure part of his northeastern frontier before he undertook the long march to Egypt, and the spring or summer of his third year would have been the logical season for a northern campaign. If he then left Mesopotamia before the rainy season began in December, the winter of that year could have seen him on the road to Egypt, whence he might return in the spring of the following year.

The biblical evidence offers some support to this outline. After construction of the temple began in the sixth month of Darius' second regnal year,¹² there follow five prophecies, two by Haggai and three by Zechariah. Four of these date to the last half of *year 2*, and, because of the threat to the rebuilding of the temple arising from the visit of Tattenai (Ushtannu) or possibly because of nationalistic aspirations, they are tinged with revolt. The last dated prophecy of Zechariah, in the ninth month of *year 4*, has no hint of revolt. In this interval the decree of Darius in favor of the Jews was issued and the march to Egypt took place, the combination of which would effectively quiet any Jewish inclination to revolt.

A small amount of other evidence, mainly Egyptian, remains to be considered. A demotic papyrus from the Golenischeff collection,¹³ a document of the temple of Horus at Edfu, records in line 15 a date for Darius of *year 3, second month of the first season* (February, 519). Preceding this date, in lines 2 and 4, there occurs *second month of the second season*, which presumably is *year 2* (June, 520). On the basis of the biblical evidence, Darius could not have been in Egypt so early in his third year, and we may suppose either that Aryandes had embarked on a policy of appeasement after he had seen the rapid rise of Darius'

"the Saka, those beyond the sea," in the west. It would appear, then, that when Darius came to the throne the only Saka of whom the Persians had knowledge were those living on the northern frontier, probably to the east of the Caspian Sea, and that it was only later, after his Indian campaign and the campaign against Scythia recounted by Herodotus, that it became necessary to distinguish by appellatives the various tribes of the Saka people. The canal stelae of Darius in Egypt, written after the Indian but before the Scythian campaign, know only eastern Saka, who are divided, according to the Egyptian, into the "Saka of the marshlands" and the "Saka of the plains." These Posener identifies with the *Sakā tigrakhaudā* and the *Sakā haumavargā*, respectively (*op. cit.*, pp. 184 f.). To account for the fact that Pers. e includes India in the empire, but fails to distinguish between the two eastern Saka, it may, however, be necessary to posit a northeastern campaign after the Indian one and before the erection of the canal stelae. On such a campaign Darius could have conquered the *Sakā haumavargā*.

¹² Hag. 1:15.

¹³ A hand copy of part of this papyrus was published by Revillout in *Rev. Eg.*, Vol. III, No. 2 (1883), Pls. 1-2. Griffith, working from photographs, assigned the document to the reign of Darius I (cf. *Ryl.* III, 25).

power or that the papyrus was written sometime later in the reign and dated retroactively.

A passage in Polyaeus¹⁴ tells how Darius arrived in Egypt during the mourning for the death of an Apis bull and offered one hundred talents for the discovery of another. Wiedemann has associated this passage with the Apis which died in the fourth year of Darius (518).¹⁵ But Apis died on the 4th of Pachons and was buried on the 13th of Epiphi, and this time of year, from August 31 to November 8, would be a most improbable time for campaigning against Egypt, with the inundation at its height in September. Cambyses, we recall, carried out his campaign in the late winter and early spring, and Darius was with him as a member of his bodyguard.¹⁶ The germ of truth in Polyaeus' story may be merely that Apis died in the same year that Darius arrived in Egypt (though that arrival was months earlier, in the winter) and that the new satrap offered a reward for a successor, in the name of Darius and in accordance with his conciliatory policy.

This stela of year 4 which records the death of Apis bears no Horus name on the banner behind the king, and Darius is entitled simply "King of Upper and Lower Egypt." This has led Posener to suggest that the stela may have been written before Darius' arrival in Egypt, at which time he presumably would have had a titulary composed for himself.¹⁷ Such a conclusion, however, is invalidated by the fact that on none of Darius' later monuments, with the exception of his temple in the Kharga oasis, is a full titulary, or even a prenomen and nomen, written.¹⁸

To summarize, then, there is no important evidence to militate against the theory that Darius left Persia in the late summer of 519 B.C.

¹⁴ vii. 11. 7.

¹⁵ *Geschichte Aegyptens* (Leipzig, 1880), p. 236. Through a blunder he gave the date as toward the end of 517, and unfortunately this error has been perpetuated by later writers, e.g., R. W. Rogers, *A History of Ancient Persia* (New York, 1929), p. 99; Swoboda, in Pauly-Wissowa, *Real-Encyclopädie*, VIII, 2189. Sarre-Herzfeld, in *Iranische Felsreliefs* (Berlin, 1910), p. 106, on the basis of the 517 date, argue that the Indian campaign took place in 518 and that work on the Persepolis terrace began 518/17. Hinz (*op. cit.*, p. 372) refers to Wiedemann in justifying his restoration of the date in Beh. § 71 (cf. n. 8 above), stating that the monument was completed before the return from Egypt. But Wiedemann's date should be 518, and Darius' fifth year from his accession to the kingship would be from September 29, 518, to September 28, 517. His fifth year according to Babylonian regnal year reckoning would be still later, 517/16. Posener (*op. cit.*, p. 181) seems not to have noticed Wiedemann's error.

¹⁶ Herod. iii. 139.

¹⁷ *Op. cit.*, p. 176.

¹⁸ Gauthier, *Le Livre des rois d'Égypte*, IV, 140 ff.; cf. also the canal stelae in Posener, *op. cit.*

on campaign against Egypt. Having reached there early in 518, and having disposed of Aryandes, he installed a new satrap and gave orders for the construction of the canal from the Nile to the Red Sea. After this he returned to Asia (in the spring[?] of 518), whence, before the end of 518, he ordered his satrap in Egypt to have the laws collected.

CHRONOLOGICAL TABLE

Event	Date B.C.	Regnal Year	Month	Day	Source
Death of Bardiya	522, Sept. 29	Acc.	VII	10	Beh. § 13
Revolt of Egypt					Beh. § 21
Last battle of Nebuchadnezzar III with Darius	Dec. 18		X	2	Beh. § 19
Defeat of Nebuchadnezzar IV	521, Nov. 27	(I)	VIII	22	Beh. § 50
Behistun inscription composed					
Ushtannu (Tattenai), first date	520, Mar. 21	I	XII	18	Strass. Dar., No. 27
Revolt of Atameta		(II)			Beh. § 71
Haggai, first prophecy	Aug. 29	II	VI	1	Hag. 1:1
Work begun on temple	Sept. 21	II	VI	24	Hag. 1:15
Visit of Tattenai to Jews, letter to Darius		II			I Esd. 6:3; Ezra 5:3
Haggai, second prophecy	Oct. 17	II	VII	21	Hag. 2:1
Zechariah, first prophecy	Nov.	II	VIII		Zech. 1:1
Haggai, third prophecy	Dec. 18	II	IX	24	Hag. 2:10
Temple foundation laid	Dec. 18	II	IX	24	Hag. 2:18
Zechariah, second prophecy	519, Feb. 15	II	XI	24	Zech. 1:7
Decree of Darius in favor of the Jews					I Esd. 6:27; Ezra 6:1
Expedition of Darius against Skunkha (<i>Sakā tigrakhaudā</i>)	(Summer)	(III)			Beh. § 74
Appendix added to Behistun inscription, by		(III)	(VII)		
Egyptian campaign of Darius	519/18 (Winter)	(III)			
Darius leaves Egypt	518 (Spring)	(IV)			
Death of Apis	Aug. 31	IV	IX	4	Louvre St. 357
Burial of Apis	Nov. 8	IV	XI	13	Louvre St. 357
Order for compilation of Egyptian laws		IV			Dem. Chronicle, v. c, 9-11

ORIENTAL INSTITUTE
UNIVERSITY OF CHICAGO

HURRIAN CONSONANTAL PATTERN¹

PIERRE M. PURVES

I

In an article published in this *Journal*² the writer set forth his discovery that the pattern of voiced and voiceless stops noted by J. Fried-

¹ The problems treated in the beginning of this article were discussed by the writer in a paper entitled "Medial Pause in Hurrian," read April 15, 1941, at the joint meeting of the American Oriental Society and its Middle West Branch in Chicago.

In addition to the abbreviations in *AJSL*, LVII (1940), 162, n. 1, the following are used:

- CBS University Museum, University of Pennsylvania. Unpublished Nippur tablets collated by A. T. Clay and cited by him in *PNCP*.
- Ch.B. Chagar Bazar; applies also to names from that locality compiled by C. J. Gadd in *Iraq*, VII (1940), 35-42.
- HSS "Harvard Semitic Series."
- Mari Hurrian tablets published by F. Thureau-Dangin in *RA*, XXXVI (1939), 1-28.
- NDA Moshe Berkooz, *The Nuzi Dialect of Akkadian: Orthography and Phonology* ("Language Dissertations," No. 23 [Philadelphia: Linguistic Society of America, 1937]).
- PNCP A. T. Clay, *Personal Names from Cuneiform Inscriptions of the Cassite Period* ("Yale Oriental Series," *Researches*, Vol. I [New Haven, Conn., 1912]).
- RS Tablets found at Ugarit (Ras Shamra). RS 1-48 published by Charles Vroilleaud in *Syria*, Vol. X (1929), Pls. LXI-LXXV, after p. 308; RS 49, *Syria*, XV (1934), 153; RS 50, *Syria*, XII (1931), 389; RS 372, *Syria*, XX (1939), 125-29.
- Tuś. The Tuśratta letter, written in Hurrian by Tuśratta, king of Mitanni, to Amenhotep III of Egypt. All citations in this article are taken from J. Friedrich's transliteration in his *Kleinasiatische Sprachdenkmäler* (Berlin, 1932), pp. 7-32.
- Ug. Voc. The Sumerian-Hurrian vocabulary discovered at Ugarit (Ras Shamra) in 1930. Copied, transliterated, and discussed by Thureau-Dangin in *Syria*, XII (1931), 234-66; transliterated again by Friedrich, *Kleinasiatische Sprachdenkmäler*, pp. 149-55.

Rev. Dr. E. R. Lacheman, of Torrington, Conn., has kindly collated the immediately available SMN material which is cited in this article with the permission of Professor Robert Henry Pfeiffer, curator of the Semitic Museum at Harvard University.

The writer acknowledges that his researches would have been impossible without the names compiled by Clay, *PNCP*, Ebeling in *MAOG*, Vol. XIII, Heft 1 (1939), and in Gadd's Ch.B. list. In many instances the writer departs from the readings in these lists, as in the case of the Ch.B. names in n. 11. The same applies to the lists compiled by Clay and Ebeling. In citations of divine and personal names, initial letters are capitalized in order to distinguish them from citations of ordinary lexical material. In readings of Hurrian material from Hurrian sources, all stops are considered as voiceless. When this same material is provided by Semitic sources, stops occurring singly after vowels and next to sonants are considered as voiced. Thus, for example, the name of the chief Hurrian deity is cited as *Teššup* when found in Tuś. and other texts of Hurrian origin. When this same deity is mentioned in texts of Semitic origin, such as those from Dilbat, Nippur, Mari, and Ugarit, the name is read *Teššub*. For ordinary purposes the former reading is preferred.

² Entitled "The Early Scribes of Nuzi," *AJSL*, LVII (1940), 162-87, hereafter referred to as "Early Scribes."

rich and C.-G. von Brandenstein³ in Hurrian texts written in the alphabetic cuneiform script of Ugarit reflected the hearing of Hurrian by Semitic scribes.⁴ The basis of this discovery was the observation that the very same pattern occurred whenever Hurrian names were written by scribes whose native language was Akkadian in its several forms.⁵ The evidence, it is recalled, was drawn from Hurrian names on tablets found at Dilbat, Nippur, and Aššur, localities where the presence of Hurrian scribes is hardly conceivable,⁶ and from names occurring on the few Nuzi tablets which were written in good Akkadian by scribes with Akkadian names.⁷

The pattern concerned is generally understood as one in which stops were voiceless when initial and when doubled but voiced medially when occurring undoubled after vowels or adjacent to the sonants *l, m, n, or r*.⁸ These rules are not without their exceptions.⁹ Since this stop pattern apparently resulted from Semitic hearing of Hurrian, it was concluded that, phonemically, Hurrian had only one set of stops, the actual phonetic difference in voicing being unrecognized by speakers of Hurrian but readily perceived by Semitic listeners to whom difference in voicing was phonemic.¹⁰

In continuing his study of the subject, the writer must first deal with a notion of his own, shown to be erroneous by evidence yielded by additional Hurrian names written by Semitic scribes on the tablets found at Chagar Bazar which have been recently compiled by C. J. Gadd.¹¹ The stop pattern is so obvious in this material that a critical

³ See Friedrich, *An. Or.*, XII (1935), 130 f., and C.-G. von Brandenstein, *ZDMG*, XCI (1937), 574.

⁴ See "Early Scribes," pp. 183 f.

⁵ *Ibid.*, pp. 172-77.

⁶ However, a Middle Assyrian scribe *Ta-gu-uḫ-li mār Ew(ri)-ri-šu-ḫur-ni* (*KAJ* 167:24) is the Hurrian son of a Hurrian father. Nevertheless, he certainly would not have been able to follow his calling at Aššur unless he had been thoroughly Assyrianized.

⁷ "Early Scribes," pp. 180-83.

⁸ See Friedrich, *An. Or.*, XII, 131; and von Brandenstein, *ZDMG*, XCI, 574; and "Early Scribes," pp. 173 f., 180. Evidence for the voicing of stops after *m* and *n* in the alphabetic texts is rare, presumably because of the tendency of *m* and *n* to disappear in writing before stops (see "Early Scribes," p. 184).

⁹ See von Brandenstein, *ZDMG*, XCI, 574, and "Early Scribes," p. 174, n. 58, and p. 182.

¹⁰ See "Early Scribes," pp. 184 f. At present the writer's opinions diverge from the view expounded by Speiser in *Language*, XVI (1940), 319-40, who maintains that difference in voice existed phonemically in Hurrian.

¹¹ *Iraq*, VII (1940), 35-42. Other Ch.B. names have been published by Gadd, *Iraq*, IV (1937), 178-85. Since hardly any of the items in this publication are involved in this ar-

examination of qualifying examples is superfluous and, therefore, provides ample testimony of the handiwork of Akkadian scribes. Included are a number of names of a certain type which would have prevented the writer from going astray in the previous study. They contain the elements *-teššub*, *-tilla*, *-tirwi*, *-lašenni* in a phonetic context which apparently called for voiced initial *d* rather than a voiceless *t*. An attempt to resolve the dilemma assumed that *d* was actually pronounced.¹² Since these elements represented Hurrian deities well known even to Semites, the Akkadian scribes were erroneously believed to have expressed their etymological knowledge in writing. Thus, for example, the Akkadian scribe was understood to have heard *-deššub*; but, supposedly aware that the element involved the deity Teššup, the scribe was assumed to have exhibited his etymological knowledge by writing *-te-šub* and the like.

However, Ch.B. names contain final elements written *-ka-na-ze/zi*, *-ke-eš-še*, *-ki-ia-ze*, *-ta-na*, and *-tu-up-ke* in context apparently calling for *-ga-na-ze/zi*, *-ge-eš-še*, *-da-na*, and *-du-up-ke*, respectively.¹³ These elements are Hurrian words with whose etymology no Semitic scribe can imaginably have been familiar.¹⁴ Overlooked also was the element

ticle, the citations to Ch.B. will not refer to it unless specifically stated. The writer departs from Gadd's readings. What Gadd reads as *u* the writer understands as *ú*. At times where Gadd reads *b* the writer reads *p*. Many signs which Gadd reads as *bi*, *ki*, *ni*, *zi*, according to custom, the writer at times reads as *he*, *ke*, *né*, *ze*. Where Gadd reads *be* the writer reads *úš*. With this understanding the reader should have no difficulty in checking the material which the writer has drawn from Gadd's invaluable contribution.

¹² "Early Scribes," pp. 177, 181 f. The writer retracts his suggestion of the value *díl* for Nippur *be/til*, *ibid.*, p. 180. In "Early Scribes," p. 179, the citation of *še-na-til-la* (VAS, VII, 147:9) was quite unwise, for it is really *še-na-be-la-šu*, an Akkadian name. The writer followed Ungnad's former misreading in *BA*, VI, Heft 5 (1909), 13 f. For the correct reading see J. Kohler and A. Ungnad, *Hammurabi's Gesetz*, V (Leipzig, 1911), (No. 1397).

¹³ As in Ch.B. *Na-wa-ar-ka-na-zi*, *Ša-du-um-ke-eš-še*, *Ša-zu-um-ke-eš-še*, *Me-me-en-ki-ia-ze*, *Pu-zu-um-ki-ia-ze*, *Aš-tu-a-ta-na*, *Ša-aš-tu-a-ta-na* (questionable, as Gadd notes), *Tu-up-ki-ta-na*, *Na-wa-ar-tu-up-ke*, where the initial consonants of the final elements, appearing as they do after vowels and sonants, are actually voiceless instead of being voiced as expected.

¹⁴ Hurrian *kešše* occurs as *ke(α)-eš-še* (KUB, XXVII, 1 ii 30, 31, 70; 6 i 31), *ki-iš-še* (KUB, XXVII 4:7; 8 rev. 7), and in the obscure [G]IŠ.SÚ.A-*hi* (KUB, XXV, 44 ii? 5), which suggests the meaning "throne." See von Brandenstein, *ZDMG*, XCI, 569. The writer does not know of the instance alluded to without reference by von Brandenstein in which *kešše* begins with *h*. Hurrian *kiaše* occurs as *ke(α)-e-a-ši* (KUB, XXVII, 42 rev. 22), *ki-i-a-ši* (VBoT, 59 ii 4, 10, iii 9), and *ke(α)-e-a-še-ne* (KUB, XXVII, 42 obv. 7, 8). It occurs as *ke(α)-ia-še-ne-wee-na-a'-[še]* in KUB, XXVII, 38 i 4. Since it bears only once the divine determinative, *kiaše* must be a divine epithet rather than a deity, as L. Oppenheim proposes in *AOF*, XII (1937-39), 36 f. For *kiaše* as a final element in feminine Nuzi names see Oppenheim, *loc. cit.* The element written *ta-na* recalls the Nuzi name *Ta-na-e*

-tatta in Nippur *Erme-tatta*,¹⁵ comparable to Nuzi **Erwi-tatta*, which should have given warning. These examples indicate that the unexpected voicelessness in initial stops in final elements of Hurrian personal names was a Hurrian speech habit faithfully recorded by Akkadian scribes.

A ready explanation for this phenomenon is to be found in the well-known Dilbat name *Te-eš-šu-ub-a-ri* (VAS, VII, No. 72:10),¹⁶ which, when divided into elements, is *Teššub-ari*. The written occurrence of the name shows that there was a pause between the elements so perceptible that the Dilbat scribe took it for an actual aleph. The procedure was very much the same at Ch.B., where aleph is expressed in the writings *Am-ma-an-e-še*, *An-da-ar-e-še*, *A-ra-an-zi-iš-a-ri*, *A-rum-a-ri*, *A-ga-ab-e-li* (<ʿ> *A-taʿ-abʿ-ē-li* in Ch.B. list), *A-we-en-ú-bi*, *E-de-en-e-li*, *Ha-zi-ib-iš-ša-ra*, *Ki-lum-al-la-i*, *Ki-ri-ib-e-li*, *Ki-ri-ib-ul-me* (*D(Kʿ)i-ri-ib-ul-me-ak* in Ch.B. list), *Ku-zu-ub-a-ri*, *Mu-zu-um-a-ri*, *Ši-in-ap-ze/sé*, *Te-eš-še-en-a-ri*, *Ú-na-ab-a-ri*, *Ú-nu-úš-ú-mar*. These names represent the respective Akkadian versions of Hurrian *Amman-ešše*, *Antar-ešše*, *Arans/išh-ari*, *Arum-ari*, *Akap-elli*, *Awen-umpi*,¹⁷ *Eten-elli*, *Hašip-išhara*, *Kelum-allai*, *Kirip-elli*, *Kirip-ulme*, *Kušuh-ari*, *Mušum-ari*, *Šin-apse*, *Teššen-ari*, *Unap-ari*, *Unuš-umar*. The aleph represented in these writings—in which signs ending in a consonant are immediately followed by signs beginning with a vowel—occurs between the elements. It shows that a distinct pause in pronunciation took place at that point. The pause occurs between the elements as if they were words or free forms. As a

(HSS, V, 104:15; *AASOR*, XVI, 48:22, 41). Apparently it is to be associated with Hurrian *tan*, "do." The word *tupke* is discernible in *tú-up-kà-e* (KUB, XII, 44 ii 22), *tup-ki-ia-wee* (KBo, V, 2 ii 23); *tú-up-ki-ni* [...] (KUB, XII, 51 ii? 9); *tú-up-ku-un-na-a-ša* (KBo, II, 21:7); perhaps in *ki-ir-tú-up-ki-na-a-ša* (*ibid.*, 1. 6). The writer possesses no information on Ch.B. *ka-na-zi* but suggests that underlying Hurrian **kanaše* is involved.

¹⁵ Written *Er-me-ta-at-ta* (BE, XIV, 56a:7; BE, XV, 37:51; 53:7; 158:2), *Er-me-ta-ta* (BE, XV, 76:5; 187:2), *Er-mi-ta-at-ta* (BE, XV, 90:42). References in Clay, *PNCP*, p. 93, where it is taken as *Irmetatta*. Evidently the dialect of the Hurrians at Nippur shared with Nuzi Hurrian the form *erwi* which is the well-known variant of Hurrian *ewri*, "king."

¹⁶ Discussed by Ungnad in *BA*, VI, Heft 5 (1909), 8 f., and in *Subartu* (Berlin, 1936), p. 140. This name brings up the annoying problem of reading *atal* as opposed to *ari* (see L. Oppenheim in *RHA*, IV, Fasc. 26 [1937], 66).

¹⁷ In the Ch.B. list, p. 36, n. 11, Gadd associates *-ú-bi* with Opis. The writer, in line with his association of *ibnkl* (RS 4:47, 48) with the goddess Umpi-nikkal in "Early Scribes," p. 184, sees in *-ú-bi* a derivation from Hurrian *umpi*. In the controversy between A. Goetze and H. L. Ginsberg (see *Orientalia* [N.S.], IX [1940], 223-28, 228 f.) the writer finds himself an ally of the latter.

matter of fact, that is precisely what they were. Accordingly, any stop occurring as the initial sound of a final element had to be voiceless according to the pattern referred to above, for it was the initial sound of an actual word and was treated as such in speech.

This procedure applies also to the Nuzi names composed with *-teššub* and *-tilla*, mentioned previously.¹⁸ Most important, the Nuzi name Ewara-tupi, written *E-ma-ra-tu-bi* by an Akkadian scribe there, turns out to be Hurrian.¹⁹ The pause between the elements kept the phonetic form *-tubi* from becoming *-dubi* as would have been normally expected otherwise. The writer, unaware of this development, tried to deal with it by a fantastic explanation which must be retracted.

When the initial element ended in a consonant, the pause in question must have been subject to adverse phonetic influences, for it was less frequent later on, during the Kassite period. Thus, for example, the Hurrian names Matip-apu and Tatip-apu occur in a Nippur text as *Ma-di-ba-bu* (PBS, II, Part 2, No. 84:8) and *Ta-di-ba-bu* (*ibid.*, l. 7), writings which openly ignore the pause between elements. In Ch.B. of the Old Babylonian period they would have been written *Ma-di-ib-a-bu* and *Ta-di-ib-a-bu*. Exceptional at Nippur are writings observing the medial pause as in the case of *Ak-kul-en-ni* (PBS, II, Part 2, No. 84:26) for Hurrian Akkul-enni. The same situation is quite evident in Nuzi, where Hurrian Tehip-apu, for example, exists under conditions in which the writing *Te-ḫi-pa-pu* vies with *Te-ḫi-ip-a-pu* and finally prevails.²⁰

¹⁸ See again "Early Scribes," pp. 177-80, 181 f. The writer still adheres to his analysis of *-de(ja) < -te(ja) < -teššub* in "Early Scribes," p. 179.

¹⁹ Written *E-wa-ra-tu-pi* (JEN 566:24; JENu 412), *I-wa-ra-tu-ū-pi* (HSS, V, 63:2). The writing *E-ma-ra-tu-bi* (SMN 3094) by the Akkadian royal scribe Apil-sin has already been alluded to in "Early Scribes," p. 183. Hurrian origin for *ewara-* seems probable in view of *e-wa-a-ra-ti* (KUB, XXVII, 29 iv 1). Dr. Robert S. Hardy has called the writer's attention to *E-wa-a-ri-ša-tu-ni* (2 BoTU 12 A i 26; 12 B i 2). These examples establish the vocalization *e-wa-ra-* or *e-wa-ri-* wherever encountered. This applies to the famous name *E-wa(pi)-ri-šar-ri*, also ideographically expressed EN.LUGAL, from Qatna (see Virolleaud, *Antiquity*, III [1929], 315, and *Syria*, XI [1930], 313, l. 44), which has been generally misunderstood as *E-w(i)-ri-šar-ri* and confused with *Ewri-šarri*.

Nuzi Ewara-tupi is certainly Hurrian. In view of Ug. Voc. ii 23, *tupi* must mean "mighty," and on the basis of the equation *ewari* = EN in the Qatna version, Ewara-tupi must mean "(My) lord is mighty."

²⁰ The first type in which medial pause is not indicated is represented in *Te-ḫi-pa-pu* (e.g., JEN 310:34, 39), of which there are at least twenty-one instances; in *Te-ḫi-pa-pu* (HSS, V, 72:55); and in *De-ḫi-pa-pu* (HSS, V, 64:14). All in all there are, then, twenty-three instances of spellings ignoring the pause as opposed to nine instances where it is indicated, as in *Te-ḫi-ip-a-pu* (see, e.g., JEN 123:21, 26). This ratio seems to hold throughout in Nuzi names where the formative *p* before vowels is concerned.

When the pause disappeared between any two consonants comprising the final consonant of the first element and the initial consonant of the last element, retrogressive assimilation tended to take place between the consonants in question. This was the fate of the formative *p*, both at Nippur and Nuzi, which was frequently subject to total assimilation to following *h*, *k*, *l*, *m*, *š*, and *t*. Before *n* the assimilation is often only partial, for the formative *p* becomes *m* in most cases.²¹

Also subject to assimilation under the same influences is the little-known formative *n*. While *p* made itself noticeable at Nuzi by its occasional resistance to assimilation by a following consonant, *n* lacked such integrity. As a result of the instability of the pause between elements during the Nuzi period, total assimilation of the formative *n* to certain following sonants and spirants was quite consistent. For this reason it has escaped the notice of most investigators. At Nuzi the formative *n* is noticeable chiefly between vowels, as in the names 'Ammin-ešše, Anin-api, Apen-ari, Arpin-ari, Erhan-ari, Erwen-ari, Ithim-ari, Kulpen-ari, Namhen-ari, Parhen-ari, Šelwen-ari, Tirwen-ari, 'Tirwen-elli, Umpin-api, and Wantin-ukur.²² Even so, the formative *n* is not readily apparent, because the spellings show that the pause between elements was well on its way out. Only once, to the writer's knowledge, is there to be found a writing which observes the pause, and that is *Nam-ḫe-en-a-ri*, in an unpublished text (SMN 2045). All other writings place *n* and the first vowel of the final element in the same sign, imparting to the casual observer the impression that final elements are ones in which the initial sound is *n*, and not the following vowel.

At Ch.B. *n* is quite evident as a suffix of initial elements in the writ-

²¹ See Berkooz, *NDA*, pp. 47 f., and "Early Scribes," p. 176, n. 66.

²² Observation of the formative *n* is to be credited to Oppenheim, *RHA*, IV, Fasc. 26 (1937), 61. The spellings in which it is involved are the following: 'Am-mi-né-ek-še (SMN 3347:2), A-ni-na-pi (JEN 71:4), A-pé-na-ri (JEN 359:18), A-pi-na-ri (JENu 529a), Ar-pi-na-ri (JEN 240:19, 22), Er-ḫa-na-ri (JEN 278:15, 21, 23), Er-ḫé-na-ri (SMN 3328:1); Er-we-na-ri (SMN 3357:6); It-ḫi-na-ri (HSS, IX, 105:5, 6), Ku-ul-pé-na-ri (HSS, IX, 23:9), Nam-ḫe-na-ri (JEN 226:41, 43), Na-am-ḫa-na-ri (HSS, V, 77:8); Na-am-ḫe-na-ri (JEN 209:25, 36 [miscopied Na-bi-ḫi-na-ri]), Nam-ḫé-en-a-ri (SMN 2045); Pár-ḫé-na-a-ri (JEN 323:6), [P]ár-ḫé-na-ri (JEN 36:2), Pár-ḫe-na-ri (JEN 36:10, 12, 14, 17), Pár-ḫa-na-ri (G 72:12), Pa-ar-ḫé-na-ri (JEN 351:5, 9), Pa-ar-ḫa-na-ri (SMN 3082, 3094, 3101), Še-el-we-na-ri (JEN 60:30), Še-il-we-na-ri (JEN 251:2, 29), Ti-ir-we-na-ri (HSS, IX, 5:6), 'Ti-ir-we-né-el-li (SMN 494:15 [in JAOS, LVII, 183]), Um-pi-na-pi (JEN 514:7), Wa-an-ti-nu-kúr (JEN 12:3), and Wa-an-ti-nu-kúr (JEN 407:28).

ten forms of Hurrian 'Awen-u(m)pi, 'Eten-elli, 'Šin-aps/ze, Teššen-ari, the writings of which have already been cited to exemplify the pause between elements. In addition, Hurrian 'Memen-kiaše and 'Šinen-šalli are formally preserved in Ch.B. 'Me-me-en-ki-ia-ze and 'Ši(?)né(?)en-ša-li. In Nuzi the existence of the formative *n* before following consonants is not so signally obvious. Before the stop *t*, it had a tendency to disappear utterly as far as writing is concerned. Thus Nuzi Umin-tanni is written variously as *Ū-mi-in-ta-an-ni* (JEN 397:6, 14; 546:5; JENu 877), *Ū-mi-en-ta-an-ni* (JEN 518:1), and *Ū-mi-ta-an-ni* (JEN 546:36). Before sonants and spirants its identity is concealed by its total assimilation to them.²³ Thus the form revealed as 'Šinen-šalli at Ch.B. becomes Šineš-šalli at Nuzi.²⁴ Initial *eten-*, as revealed by Ch.B. 'Eden-elli, becomes *etem-* in Nuzi 'Etem-menni²⁵ and *eteš-* in the very common Nuzi name *Eteš-šenni*.²⁶ It was suggested above that Ch.B. 'Amman-ešše probably resulted in the form 'Ammin-ešše at Nuzi. Under these circumstances Nuzi 'Ammiš-šalli²⁷ can safely be regarded as a development from *'Ammin-šalli if not ultimately from *'Amman-šalli.

In addition to its becoming weakened, there were conditions under which the pause between elements was completely eliminated. In each instance this loss occurred when the name was shortened. For instance, the final element *-teššup* assumed the hypocoristic form *-teja* and the shortened form *-te* as well.²⁸ At Nippur, *-teja* and *-te*, when following an initial element ending in a sonant or a vowel, were found to occur as *-deja* and *-de*.²⁹ In other words, the pause between elements was lost and the stop after a vowel became voiced in accordance with the stop pattern. This process may be observed in Ch.B. of the Old

²³ But remains unassimilated in Nuzi Šatin-šub and Tain-šub. The writer suspects that *n* in these examples is a derivation from some other sound, presumably *m*; cf. 'A-ri-im-tu-ri (HSS, V, 76:4, 6) and 'A-ri-in-tu-ri (HSS, V, 11:1), both linked by the same genealogy.

²⁴ Written 'Ši-ni-iš-ša-el-li (HSS, IX, 38:30), 'Ši-ni-ša-al-li (SMN 17), 'Ši-ni-iš-šal-li (SMN 50), 'Ši-ni-iš-šā-al-li (SMN 135), 'Ši-ni-ša-li (SMN 214), 'Ši-né-eš-šā-al-li (SMN 395), 'Ši-ni-iš-šā-li (SMN 642), 'Ši-ni-šal-li (SMN 654).

²⁵ Written 'E-te-em-me-en-ni (HSS, IX, 38:34).

²⁶ Mostly written *E-te-eš-še-en-ni* (e.g., JEN 6:18).

²⁷ Written 'Am-mi-iš-šal-li (SMN 394); 'Am-mi-šā-li (SMN 126).

²⁸ On which see Oppenheim, WZKM, XLIV, 203 f., n. 1, and writer, JAOS, LVIII (1938), 465 ff.

²⁹ "Early Scribes," p. 179.

Babylonian period, where Nuzi 'Twi-tilla³⁰ can be shortened to 'Twi-dil, as attested by the Ch.B. writing 'I-wi-di-il. But, as this writing shows, the shortening of *-tilla* to *-til* was similarly accompanied by voicing of the dental owing to the absence of the expected pause between the elements.

The final vowel of an initial element was frequently lost, presumably by syncope. When that vowel happened to be *a*, the pause between elements was lost along with it. Such shortening is attested at Nuzi where the element *enna-* frequently becomes *en-* or *enn-* as in the name Enna-šukru which has a variant En-šukru.³¹ By this token the Nuzi writing *Še-en-tā-ti* (SMN 3115) must express a similar shortening of the Nuzi name Šenna-tati.³² But the Akkadian version of this shortened form is *Še-en-da-da* (CBS 3480).³³ Here the loss of *a* is accompanied by absence of the pause between elements, a development resulting in the voicing of the dental stop after *n* according to pattern. In his last article the writer noticed but inadequately explained the Nippur writing *Ha-bi-ir-di-il-la* (PBS, II, Part 2, No. 89:2) corresponding with the Nuzi name Hapir-tilla.³⁴ In view of the shortened form Šen-dada, Nippur Hapir-dilla must have been a shortening of original *Hapira-tilla. This seems quite likely, in view of Nuzi Hapira.³⁵

II

The process just discussed has ramifications which lead the investigation into a much broader field. The Nippur writing *En-zu-ug-ri* (CBS 3480)³⁶ obviously corresponds to Nuzi En-šukru³⁷ which, as demonstrated above, is a modification of Enna-šukru. However, else-

³⁰ Written 'I-wi-ti-il-la in SMN 403, 3230. Shortening of names ending in *-lla* by the dropping of *-la* is demonstrable in SMN 2597, where the name 'Šarum-alla, written 'Ša-ru-ma-al-la, variously occurs as 'Šarum-al, written 'Ša-ru-ma-al.

³¹ Cf., e.g., *En-na-šuk-rū* and *En-na-šuk-ru* (JEN 424:2, 11, 14, 16) and *E-en-šuk-rū* (JEN 584:26, 43), all with the same genealogy. See also Berkooz, NDA, p. 29.

³² Written 'Še-en-na-ta-ti (RA, XXVIII, 7:15, on p. 38).

³³ Collated by the writer at the University Museum in Philadelphia.

³⁴ See "Early Scribes," pp. 172, 173. Cf. Nuzi *Ha-pi-ir-ti-il-la* (HSS, V, 55:38, 41). The writer retracts his unsuccessful attempt ("Early Scribes," p. 180) to explain the phonetic activity involved.

³⁵ Written, e.g., *Ha-pi-ra* (JEN 6:16).

³⁶ Collated by the writer at the University Museum in Philadelphia. Transliterated *En-(Bēl)-zu-ug(uk)-ri* in Clay, PNCP, p. 75.

³⁷ See "Early Scribes," p. 175, n. 60.

where at Nippur Hurrian *šukr* occurs with initial *š* and not initial *z*, for Nippur *Šu-gur-te-šub* (PBS, II, Part 2, No. 84:10) corresponds to the identically written Nuzi name *Šukur-teššup*, which in turn proves to be a variant of Nuzi *Šukri-teššup*.³⁸ Akkadian texts from Mari express this name as *Šu-ug-rù-te-šu-ub*.³⁹ Since it has been shown that initial stops of final elements become voiced when the initial element is thus shortened, it stands to reason that the sibilant which consistently appears in native Hurrian sources as *š*,⁴⁰ had a voiced aspect which, while not phonemic to Hurrians, struck the ear of the Babylonian scribe as a somewhat different sound approximating Akkadian *z* or perhaps *ṣ*. Similar to En-zugru of Nippur is Ch.B. *ʾAš-tu-za-ar*, which corresponds to Nuzi *ʾAštua-šar*.⁴¹ The shortening from original *ʾAštua-šar* in Ch.B. itself becomes quite evident in view of Ch.B. *ʾAš-tu-a-la-na*.⁴² Thus the pattern of voiced and voiceless sounds, which so far in this article has been confined to stops, appears to embrace sibilants and, by the same token, spirants also.

Such a state of affairs undoubtedly may have been noticed by the reader in Part I of this article, where the Ch.B. elements *ḥazib-*, *kuzuḥ-*, and *muzum-* were compared with Hurrian *ḥašip-*, *kušuh-* and *mušum-*. In addition, the Ch.B. element *-kiyaze* also alluded to cannot be disassociated from Hurrian *-kiaše*.⁴³ But Ch.B. names furnish also the elements *-muze*⁴⁴ and *-nirze*,⁴⁵ identical with Hurrian *-muše* and

³⁸ Cf. *Šu-kúr-te-šup* (SMN 3657) and *Šuk-ri-te-šup* (AASOR, XVI, 55:2, 4, 7, 8 ff.), with the same genealogy.

³⁹ See Dhorme, *RA*, XXXV (1938), 184, and *Comptes rendus* (1937), p. 3. The writer is indebted to Dr. Gelb for these references.

⁴⁰ That is, in texts which can be shown to be written by the Hurrians, such as the Nuzi documents, and above all the Tušratta letter. Probably all the Hurrian texts in Boğazköy were written by Hittites, a consideration making advisable some caution in dealing with these sources. However, on the possibility that the Hittite syllabary is truly representative of the Hurrian phonetic system see the last section of this article.

⁴¹ Written *ʾAš-tù-a-šar* (TCL, IX, 22:4). Possibly *ʾAš-tù-a-ka* (G 82:25) is a poor writing of this form.

⁴² To be divided *ʾAštua-tana*; cf. Ch.B. *Tupki-tana*, written *Tu-up-ki-ta-na*.

⁴³ Semitized form *kiyaze* occurs in Ch.B. *ʾAt-tap-ki-ia-ze*, *ʾMe-me-en-ki-ia-ze*, *ʾPu-zu-um-ki-ia-ze*, and *ʾU-nu-úš-ki-ia-ze*. See n. 14 for *kiaše* in Hurrian texts.

⁴⁴ In Ch.B. *ʾA-we-eš-mu-ze*, to be directly associated with Nuzi *ʾAweš-muše*, written *ʾA-wi-iš-mu-še* (JEN 501:19) and *ʾA-we-eš-mu-še* (SMN 352). On *muš* see Thureau-Dangin in *RA*, XXXVI (1939), 22 f.

⁴⁵ In Ch.B. *ʾAt-ta-i-ni-ir-ze*, Nuzi names with this element are *Erwen-nirše*, written, e.g., *Er-we-en-ni-ir-še* (JEN 29:7, 42); *Tai-nirše*, written *Ta-i-ni-ir-še* (JEN 487:25, 35); and *Šilip-nirše*, written *Št-lip-ni-ir-še* (SMN 403, 599). Cf. perhaps *ni-i-ir-ša-e* (Tuš. iv 66).

-nirše, along with *puzum(-)*, *šazum-*, and *-uzuwe*, which are perhaps identical with Hurrian **pušum-*, **šašum-*, and **ušue*, respectively.⁴⁶ In addition, Hurrian *šunš* in the Nuzi name *ʾŠunšun-na-ia*, written *ʾŠu-un-šu-un-na-a-a* (SMN 3506), appears as *šunz* in the Ch.B. form *ʾŠu-un-zu-na-ia*.

But this treatment of the sound which the Hurrians represented by *š* is not limited to the Ch.B. scribes when it occurs after vowels and sonants. The Nippur name *Arik-kazu*, discussed in a previous article, now seems to represent underlying Hurrian **Arip-kašu*.⁴⁷ Furthermore, Hurrian names from Nippur exhibit evidence that the sibilant which the Hurrians wrote with *š* was subject to voicing when it occurred beside *n* or *r*, just as the stops were. Thus Nippur *Še-er-zi-ia* (CBS 3480:22)⁴⁸ is obviously identical with Hurrian *Šeršija*,⁴⁹ while Hurrian *-mušni*⁵⁰ is represented at Nippur in *Aram-muzni*.⁵¹ That it was attempted to represent this sound by *ṣ* is shown by the Nippur writing *Ḥa-ši-ib-til-[a]* (PBS, II, Part 2, No. 84:22, 35), which is almost beyond doubt identical with *Ḥašip-tilla*.⁵²

⁴⁶ Ch.B. *ʾPu-zu-um* and *ʾPu-zu-um-ki-ia-ze* contain *puzum*, possibly reflected in Nuzi *Puš-teja*, written *Pu-uš-te-ia* (G 72:3); *ʾAla-puša*, written *ʾA-la-pu-ša* (JEN 174:3); *ʾHalpa-puša*, written, e.g., *ʾHal-pa-pu-ša* (AASOR, XVI, 42:3, 8 ff.); and *Puškika(?)*, written *Pu-ši-ik-ka-i(?)* (JEN 301:21). For *šazum* in Ch.B. *Ša-zu-um-šar(-ri)* cf. *šašu* in *Šašu-teššup*, written *Ša-šu-te-šup* (SMN 3241:6) and *šaš* in Nuzi *ʾŠaš-kiyāše*, written *ʾŠa-aš-ki-ia-še* (JEN 218:3, 5, 8, 11); *Šaš-naihe*, written *Ša-aš-na-i-hé* (JEN 218:1); *Šaš-tae*, written *Ša-aš-ta-e* (e.g., JEN 383:1, 5 ff.). Cf. also *ʾŠašua*, written *ʾŠa-šu-ú-ia* and *ʾŠa-šu-ia* (JEN 445:1, 5).

Ch.B. *ʾPa-aḥ-ri-ú-zu-we* and *ʾTa-da-ab-ú-zu-we* contain a final element unknown in Nuzi names to the writer, who tentatively takes recourse to *u-šu-ni* (KUB, XXVII, 21:3), *u-šu-u-ni* (KUB, XXVII, 4:4; 8 rev. 4), *u-šu-un-na-a-ši-na* (KUB, XXVII, 42 rev. 12), and *u-šu-un-ni-pi-na* (KUB, XXIX, 8 iii 31).

⁴⁷ See "Early Scribes," p. 176, concerning the Nippur name variously written *A-ri-ka-zu* (PBS, II, Part II, 9:7), and *A-ri-ik-ka-zu* (ibid., 132:7). An explanation of Nippur *kazu* may be sought in Nuzi *kašu*, as in *Kašum-menni*, written *ʾKa-šu-um-me-en-ni* (G 9:2, 6 ff.). Relationship with Nuzi *Kazuhhe*, written *Ka-zu-uh-hé* (JEN 501:26), *Ka-zu-uh-he* (JEN 557:4, 14), *Ka-zu-uh-hé* (HEN 624:3, 15), is not so much obvious as apparent, for in Nippur it may perhaps have been written **Ka-zu-uh-he*; see Part IV of the present article. The writer takes this opportunity to disavow his association of Nippur *kuša* in "Early Scribes," p. 177, n. 68, with the deity *Kuṣuh*. He now sees in it underlying *kušša*.

⁴⁸ *Še-ir-zi-ia* in Clay, *PNCP*, p. 131, collated by the writer at the University Museum in Philadelphia.

⁴⁹ Various written *Še-er-ši-ia* (e.g., JEN 23:25, 43), *Šer-ši-ia* (e.g., JEN 246:16, 26), *Še-er-še-ia* (JEN 47:23), *Še-er-ši-a* (JEN 119:23). Cf. Hurrian *še-er-še* (KUB, XXVII, 38 ii 12, 18).

⁵⁰ On the element and word *mušni* see Thureau-Dangin, *RA*, XXXVI, 22 f.

⁵¹ Written *A-ra-mu-su-ni* (PBS, II, Part II, 110:9). On the conflicting evidence of *A-ra-mu-su-ni* from Tell Atchana see n. 58.

⁵² Very common at Nuzi, where it is written throughout as *Ḥa-ši-ip-til-la* (see, e.g., JEN 85:35, 39).

In "Early Scribes" the writing of the Hurrian texts at Mari was attributed to native Semitic scribes, for there the stop pattern predominates throughout.⁵³ Consequently, the sibilant uniformly represented by the Hurrians as *š* should and does occur as *z* in Mari when intervocalic. Thus *ha-za-aš-la-ri* (Mari 3:18, 19) seems to be formed on Hurrian *haš*, "listen." But conclusive is *ki-ia-ze-ni* (Mari 5:19), which, like Ch.B. *kiāze* alluded to above in this section as well as in Part I, cannot avoid comparison with Hurrian *kiaše*. Likewise *pa-za-la* (Mari 6:12) seems to be Hurrian **pašala*,⁵⁴ and *pu-za-al* (Mari 6:11), like Ch.B. *puzum*, seems to reflect Hurrian *puš*. Hurrian *taše(n)n*⁵⁵ is quite certain in *ta-ze-né-e* (Mari 1:7), *ta-ze-né* (Mari 1:9), *ta-ze-ni-ti* (Mari 1:8). Hurrian *tiš*, "heart," may very well be sought in *ti-za-da!* (Mari 6:5), *ti-za-we* (Mari 6:3), and *ti-zi-in-ni-¹en* (Mari 6:6). Finally, *wa-zu-um* (Mari 3:23, 24) is clearly formed on Hurrian *waš*.⁵⁶

Interpretation of this Hurrian sibilant as *š/z* was not constant, for the Akkadian scribes at Nuzi mostly transcribe it as *š*.⁵⁷ In other rare

⁵³ "Early Scribes," p. 184.

⁵⁴ The root evidently being *paš*; cf. *pa-ša-a-e* (KUB, XXIX, 8 iii 14), *pa-ša-la-a-e* (KUB, XXIX 8 iii 14), *pa-a-ša-na-e* (KUB, XXIX, 8 iii 5, 14), *pa-a-ši* (KUB, XXIX 8 iv 24), *pa-a-ši-pa* (KUB, XXIX, 8 iii 36, iv 18), *pa-a-ši-ta* (KUB, XXVII, 42 rev. 17), *pa-a-ši-tù* (KUB, XXVII, 42 obv. 11), *pa-a-ši-ù-z* [. . .] (KUB, XXVII, 34 iv 7).

⁵⁵ Perhaps a derivation of Hurrian *taše*. Cf. *ta-še-ni* (KUB, XXIX, 8 iv 3); *ta-še-e-né-e-we* (Tuš. i 91, 92, 99, 104); *ta-še-e-en* (Tuš. i 90); and *ta-še* (Tuš. i 85; KUB, XXVII, 1 ii 10, 11; 6 i 19); *ta-a-še* (KUB, VII, 56 i 22); and, finally, *ta-še-e-e-na^{MEŠ}* (Tuš. i 88). Another derived form occurs as the element *tašenni*, apparently identical with a place name and frequent in Nuzi Hurrian names (see Oppenheim, *AOF*, XII, 39; Purves, *JAOS*, LVIII [1938], 463, and "Early Scribes," p. 177 and n. 69). Oppenheim (*loc. cit.*, n. 55) and writer (*loc. cit.*) interpreted Nippur *Hu-di-ti-še-en-ni* (CBS 3480 iv 8) as derivation from underlying *Hutip-tašenni*. However, if such were the case, *Hu-di-ti-qi/zi-en-ni* would be the expected writing, and the suggested interpretation is perhaps wrong. Collation of the Nippur name in Philadelphia revealed it to be correctly transliterated by Clay (*PNCP*, p. 80). No opinion on this form can be ventured at present.

⁵⁶ Cf. *wa-a-ša* [. . .] (KUB, X, 63 ii 13), *wa-a-ši* (KUB, XXVII, 44:2), *wa-ša-i-na-an* (Tuš. iii 33), *wa-še-e-we* (Tuš. iii 112), *wa-a-aš-na-e* (Tuš. iv 64), *wa-a-a-šu* (KUB, XXIX, 8 ii 38), *wa-a-šu-ul-[i . . .]* (KUB, XXVII 37:10), *wa-a-a-šu-uš* (KUB, XXIX, 8 ii 43, 44). In Nuzi names cf. *Waš-elli*, written *Wa-še-el-li* (JEN 432:3, 6, 14 ff.); *Menni-waše*, written, e.g., *Me-en-ni-wa-še* (SMN 347, 635); and *Uwur-waše*, written, e.g., *U-wu-ur-wa-še* and *U-wu-ur-wa-še-e* (SMN 347, 360).

⁵⁷ Perhaps because the Hurrian environment at Nuzi accustomed them to the true nature of the phoneme. *Tāb-milk(i)-abi* betrays his original background by what must have been a reversion to natural inclination by writing *Ha-zi-ib/ip-sē-[la]- < ak > -ku* (JEN 568:31), a form which must express the underlying hybrid name **Hašip-silakku*. Cf. the Nippur Akkadian name *Ardi-Ši-la-ak-ku* (CBS 11831 in Clay, *PNCP*, p. 58). Elsewhere in JEN 568 *Tāb-milk(i)-abi* does not revert to type, since he writes *Ha-ki-ia* in ll. 1, 8, and 11 instead of *Ha-zi-ia*, which appears in JEN 570:32, a tablet written by the unknown Akkadian scribe of Nuzi (on whom see "Early Scribes," pp. 171 f.).

In "Early Scribes," p. 184, the writer suggested that Ug. Voc. was written by a Semitic scribe in view of the stop-pattern which dominates it. But there is not the expected *š/z*

instances it seems to occur as *s*.⁵⁸ However, the distinction between the voiced and voiceless phases of the Hurrian sibilant known as *š* is strictly observed in the Hurrian texts written in the alphabetic script of Ugarit. In them the stop pattern was first observed, and on that basis the writer attributed them to native Semitic scribes.⁵⁹ In the Ugarit texts the sibilant known as *š* is alternately written with the puzzling sign generally transcribed as *z*⁶⁰ when the pattern calls for a voiced sound, and with the one recently termed *θ*⁶¹ when the pattern

variation. Perhaps the scribe who wrote it was a Hurrian attempting to emulate his Semitic preceptors in following the stop-pattern. In Nuzi a parallel is to be found in the style of Šumu-libši, son of Šamaš-bāri, discussed in "Early Scribes," p. 186. Yet again the Ugarit scribe may have been a Semite, who, like his compatriots at Nuzi, correctly understood the nature of the Hurrian *š*.

⁵⁸ The name apparently written *Ha-si-AN.PA-ri-gi* (KAJ 192:12) may reflect Hurrian *haš*. A good variant of Nippur *Aram-muzni*, written *A-ra-mu-uz-ni* (*PBS*, II, Part II, 110:9) and reflecting Hurrian **Aram-mušni*, was brought to the writer's attention by Dr. Gelb. It is evidently written *A-ra-mu-su-ni* and occurs on a Tell Atchana tablet (see Sidney Smith, *Antiquaries Journal*, XIX [1939], 46), emanating from the Old Babylonian period. The writer is unable to offer an opinion on the factors involved in this evidence.

⁵⁹ "Early Scribes," pp. 183-85.

⁶⁰ Sign No. 27 in Gordon's list, *An. Or.*, XX (1940), 11. For discussions on this sign see Ginsberg and Maisler, *JPOS*, XIV (1934), 244 ff.; Harris, *JAOS*, LV (1935), 95-100; Friedrich, *An. Or.*, XII (1935), 129 f.; Speiser, *JAOS*, LVIII, 175-93; and Gordon, *loc. cit.*, paragraphs 4.1, 4.4, and 4.22. The writer uses the transliteration *z* employed by Harris. Its exact phonetic nature is not yet understood, but it has been shown that it varies with Ugaritic *z* in the local word for "breast" (see, e.g., Speiser, *op. cit.*, p. 178). This agrees with the demonstrable correspondence of *z* with *z/š*, employed by Babylonian scribes to represent the voiced phase of the sibilant appearing as *š* in Hurrian texts.

The writer diverges from the current view that *z* has basically a Hurrian origin. Although it occurs chiefly in Hurrian words, the fact remains that it does occur in Semitic words and may have had a Semitic origin. It represented perhaps an Ugaritic phoneme undergoing a change making it merge finally with Ugaritic *d*. See, e.g., Harris, *Development of the Canaanite Dialects* ("American Oriental Series," Vol. XVI [1939]), p. 36, and then Gordon, *loc. cit.*, paragraph 4.1, where a derivation from Arabic *dal* is discussed. Curiously enough, its voiceless counterpart in the Hurrian texts from Ugarit is *θ*, which is cognate with the corresponding Arabic voiceless sound *ṭa* (see following note).

⁶¹ Sign No. 29 in Gordon's list, *An. Or.*, XX, 11. The writer follows Speiser, who proposed *θ* in *JAOS*, LVIII, 175-79, to represent this sign. Its phonetic character is still a matter of debate. The underlying sound was a distinct phoneme in Ugaritic corresponding to the sound merging with *š* in the West Semitic languages spoken to the south of Ugarit (see Harris, *op. cit.*, pp. 40 f., and A. Goetze in *Language*, XVII [1941] 168 f.). However, that is because Ugaritic has a closer affinity to East Canaanite or Amurrite (see Goetze, *Language*, XVII [1941], 127-38), which also preserves the distinction between primitive Semitic *t* and *š*. The idea still prevails that *θ*, since it was a separate phoneme, had a dental rather than a sibilant character. However, the voiceless phase of the sound which appears in Hurrian writing as *š*, while rendered as *θ* by the Ugarit Semites, is variously rendered by *s* and *š* by Nippur scribes. In addition, *θbl* in the Hurrian text RS 4:27, 33 seems to vary with *abl*, *ibid.*, l. 8. This speaks more for a sibilant than for a dental phonetic effect. In fact, the *s* in the Nippur writings referred to suggests that Akkadian *šin* may at times have veered to *šin*.

Furthermore, the voiced counterpart of the sound represented as *θ* in Ugarit and as *s/š* at Ch.B. and Nippur is *q/z* at Ch.B. and Nippur. This relationship then seems to be,

calls for a voiceless sound. Ugarit *z*, then, bears the same relationship to *θ* as *z/š* does to *š* in Hurrian names and words written by Babylonian scribes. Thus intervocalic *š* is expressed as *z* in the verbal morpheme *-uša* in *ʾaržln* (RS 30:2, 4; 372 rev. 7, 15), *ʾaržnnk* (RS 4:9, 16, 28, 34), and *tžžnnk* (RS 4:49), which represent formations on *aruša-* and *tehuša-*, respectively.⁶² Similarly, Hurrian *ennaša*⁶³ results in *ʾenž* (RS 4:60, 61). The phrase *hašari hašulieš*, so frequently met in the Boğazköy texts, has long since been associated with *hžr hžlž* (RS 4:1, 6, 10, *et passim*).⁶⁴ Also in the same sources is the deity *Wišaišaphi*, appearing in Ugarit as *Pžžph* (RS 4:35, 37) and *Pžžphnd* (RS 50:5).⁶⁵ The voicing of the sibilant after *r* is to be observed in Ugarit *ʾIržp* (RS 4:41) and *ʾIržpn* (RS 4:42), which represent the deity known as Iršappi.⁶⁶ Beside *m* it is apparently voiced in accordance with the pattern, for the name *ʾAžmny* (RS 14:2), while it recalls to Harris the deity *Ešmun*,⁶⁷ is also strongly suggestive of the Nuzi name *ʾAšmun-naia*.⁶⁸

While the voiced aspect of *š* is approximated by *z/š* by Akkadian scribes and *z* by the Semitic scribes at Ugarit, the voiceless counterpart is rendered generally by *š* by the former and *θ* by the latter. This is clearly illustrated by the occurrences of *š* and *θ* in initial positions,

broadly speaking, one between the sibilants *s* and *z*. The Hurrians also had another sibilant which the writer expresses as *s* (see Part IV of the present article), since it appears as such at Nippur. Just what the phonetic distinction was between these two Hurrian phonemes is not clear. They must have been similar, for they interchange.

⁶² For *arž-* etc. see Speiser, *Language*, XVI (1940), 322. The analysis of *tžžnnk* is one on which Professor Speiser and the writer found themselves in agreement during a conversation a few months ago. Also to be considered is *hžžnnk* (RS 4:14, 21, 37, 46, 59), which must be Hurrian **hutuša*. The verbal element *hut* is very common in Hurrian names.

⁶³ Cf. *ennaša*, written DINGIR^{MEŠ}*na-a-ša* (KUB, XXVII, 39 obv. 3; 42 obv. 33; 43:8; KUB, XXIX, 8 ili 54, iv 26, 29). Cf. also *ennaši*, written DINGIR^{MEŠ}*na-a-ši* (KUB, XXVII, 42 obv. 14).

⁶⁴ By J. Friedrich in *AOF*, X (1935-36), 295. See also C.-G. von Brandenstein, *ZDMG*, XCI, 559. For a penetrating exposition of *-lieš* and its function see Gotze in *RHA*, V Fasc. 35 (1939), 103-8.

⁶⁵ Association made by B. Hrozný, *AOr*, IV (1932), 120 f., and Friedrich, *An. Or.*, XII (1935), 127 and n. 2, and 129 f.

⁶⁶ See Friedrich, *An. Or.*, XII, 129, 130 f. The Ugarit personal name *ʾEwēr* (l. 1 of tablet published by E. Dhorme in *Syria*, XIV [1933] 235-37) is generally associated with *Ewari-šarri*, misunderstood as *Ew(i)ri-šarri*, on which see n. 19. Normally *ʾEwēr* is expected. The writer feels that normal *Ewari-šarri* had become *Ewar-šarri* at Ugarit and that the *š* had become voiced as it had in *En-zugri* and *ʾAštu-zar*. It is probable that the Nuzi element *ewara-* is identical with *ewari-* in the name under discussion.

⁶⁷ See *JAOS*, LV, 98.

⁶⁸ Written *ʾAš-mu-un-na-a-a* (SMN 1251).

where the pattern calls for voiceless sounds. The evidence to be drawn from the sources utilized in this article is so abundant and so conclusive that investigation of the matter is hardly necessary here.

However, before passing on, it is quite necessary to remark that the voiceless aspect of the spirant in question, although mostly rendered by *š/θ* by Akkadian and Ugaritic scribes, undergoes a phonetic deviation influencing them at times to render it by signs containing Semitic *s*. Hurrian *šamp* as portrayed by Šampiia⁶⁹ and Kel-šampa⁷⁰ at Nuzi is to be found in Sambi⁷¹ and Sa(m)biḫ-ari⁷² at Nippur. The name Šellu at Nuzi is Sellum or Selli⁷³ at Nippur. Unless *sr* is to be read *ši* at Ch.B., the theophorous element *-šimika* appears to be *-simiga* in the Ch.B. name *ʾHa-zi-ib-si-mi-ga*.⁷⁴ Hurrian **šinenni*, apparently formed on *šinen*, as, e.g., in Ch.B. *ʾŠinen-naia* and *ʾŠinen(?)*-šalli, results in the name Sinenni at Nippur.⁷⁵ Hurrian *šumm*, as represented perhaps in *šu-um-mu-un-ni-we* (KUB, XXVII, No. 1 ii 10), *šu-mu-ni-we*, "of the hand" (Ug. Voc. ii 3), and in the Nuzi names Šummiia⁷⁶ and Šummi-šenni,⁷⁷ appears to Akkadian ears as *Sul-um-še-en¹-ni* (BE, XIV, No. 14:8),⁷⁸ at Nippur and *Sum-mi-te-šu-ub* (KAV 30b:5)⁷⁹ at Aššur and perhaps lexically as *sú-mu-un* (Mari 4:28). The same variation finds expression perhaps in *θbl* (RS 4:27, 33) and *sbl* (*ibid.* l. 8).⁸⁰

Doubling of the spirant, reflected in Hurrian writing as *šš* is also frequently defectively written as a single *š* by Akkadian scribes. This laxity has caused the name of the deity Teššup to be universally misunderstood as Tešup. The correct version is given in the Tušratta let-

⁶⁹ Written *Ša-am-pi-ia* (JEN 243:21; 289:29).

⁷⁰ Written *Ke(ri)-el-ša-am-pa* (AASOR, XVI, 95:14).

⁷¹ Written *Sa-am-bi* (BE, XIV, 12:2).

⁷² Various written *Sa-bi-ḫa-ri* (PBS, II, Part II, 90:6) and *Sa-am-bi-ḫa-ri* (BE, XIV, 12:9).

⁷³ The Nuzi form is written *Še-el-lu* (JEN 151:24; CT, II, 21:23, 24). Cf. Nippur *Se-el-lum* (BE, XV, 194:4) and variant *Se-el-li* (*ibid.*, 198:93).

⁷⁴ Unless *ʾHa-zi-ib-ši-mi-ga* is the correct reading.

⁷⁵ Written *Si-né-en-ni* (CBS 4572), cited as *Si-ni-en-ni* in Clay, *PNCP*, p. 125.

⁷⁶ See, e.g., *Šum-mi-ia* (JEN 5:19, *passim*) and *Šu-um-mi-ia* (JEN 211:15).

⁷⁷ See, e.g., *Šum-mi-še-en-ni*, AASOR, XVI, 29:29.

⁷⁸ *Lu-um(ap)-še-en-ni* in Clay, *PNCP*, p. 103.

⁷⁹ The writer abandons the reading *Šum-mi-te-šu-up* in "Early Scribes," p. 178.

⁸⁰ As noted also by Speiser, *JAOS*, LVIII, 177, but not necessarily an error as Speiser states. Perhaps underlying form is something like *ʾšap¹-la* (KUB, X, 63 ii 13).

ter, where it is written ^d*Te-e-šš-šu-pa-aš* (Tuš. i 76, ii 65, iv 118) and ^d*Te-e-šš-šu-u-up-pē* (Tuš. ii 77). When not expressed ideographically in the Hurrian texts from Boğazköy, it is written ^d*Te-e-šš-šu-up-pi* (KUB, XXVII, No. 38 ii 14, 20; iii 2, 4, 6). As the already cited Dilbat name ^d*Te-e-šš-šu-ub-a-ri* attests, the actual quantity of the double consonant was perceptible enough to be significant to the Akkadian scribe who wrote it. This is rather unusual, for, as in the Mari writing *Te-šu-ba-am* (Mari 1:34), the single *š* indicates that scribes did not understand that actual length was involved.

That writings in *šš* reflect quantity has already found exemplification in Part I with regard to Nuzi names in which the formative *-n* is assimilated to a following *š*. Similar evidence is yielded by Ch.B. *A-ra-an-zi-iḫ-a-ri*, also mentioned in Part I. The underlying Hurrian form of the initial element would seem to have been **aransih*, but quite possibly it may have been **aranših*.⁸¹ Whatever it was, the *-n* evidently assimilated to the following consonant with the resultant Nuzi form *arašših*.⁸²

In Ch.B., *ešše* (as in 'Amman-ešše) is written *-e-še*. The Nuzi elements *-šalli* and *-elli* appear in Ch.B. as *-ša-li* and *-e-li*.⁸³ The Mari texts show some evidence of this misconception. That *aššenel* may be the underlying form for *'a'(?)-še-nē-el* (Mari 4:25) is suggested by the writing *a-aš-še-iš* (KUB, VII, No. 56 ii 19) from Boğazköy. Likewise *iš-ši-na-a-an* (Tuš. iii 66) implies underlying *iššamma* for *i-ša-am-ma* (Mari 1:30 and 2:14, 15), since both seem to occupy the same syntactic position in the sentences in which they occur. More acceptable evidence is revealed when *pa-ši-ib* (Mari 1:3, 6, and 2:9) is confronted with *pa-aš-še* (Mari 6:20). Likewise *pi-ši-di-in* (Mari 5:16) appears as a defective writing of the phonetic form *piššidin* when compared with *pi-iš-ši-la*, two lines above in the same document.

⁸¹ Which recalls the river Aranzuh, so Dr. Gelb reminds the writer. Cf. equations Arans/zuh = Idiqlat (in) Subartum/Iamutbal in synonym list published by W. von Soden, ZA (N.F.), IX (1936), 235, l. 44, and Aransuh = Idiqlat (in) Iamutbal in synonym list published by idem, Die lexikalischen Tafelserien der Babylonier und Assyrier in den Berliner Museen, II (Berlin, 1933), No. 8126. See also H. G. Güterbock, ZA (N.F.), X (1938), 84, n. 1, on this and the Hittite form *A-ra-an-za-ḫu*. Hurrian **Aranših* with a *s/š > z* variation under pattern conditions seems very likely.

⁸² In Arip-arašših, written *A-ri-ip-a-ra-ši-iḫ* (SMN 20), and *[Ḫa]šip(?) -arašših*, written *[Ḫa]šip(?) -ḫi-ip-a-ra-aš-ši-iḫ* (JEN 659:39). In the Nuzi rendition of Arip-arašših note the failure to write double consonants when they are required.

⁸³ See Ch.B. *'A-ḡa'-ab'-e-li* (transliterated *A-ta(?) -ab(?) -e-li* in Iraq, VII, 36), *'E-de(?) -en-e-li*, *'Ki-ri-ib-e-li*, *'Nu-bur-e-li*, *'Ta-qa-al-e-li*, *'Ši(?) -nē(?) -en-ša-li*, *'U-nu-ū-ša-li*.

That quantity is involved becomes quite evident when *ú-šu-ul* (Mari 1:2) is compared with *uš-šu-li-e* (KUB, XXIX, No. 8 iii 51) and *u-uš-šu-li-e-eš* (KUB, XXVII, No. 42 rev. 24) from Boğazköy. The defective writing *ú-šu-um* (Mari 2:18) is clearly identical in form with *uš-šu-u-um* (KUB, XXVII, No. 38 iii 4, 6, 7, 10, 12) from Boğazköy Hurrian. The quantitative character of the sibilant in *wa-šu-me* [. . .] (Mari 3:26) becomes clear upon comparison with the Boğazköy Hurrian forms *wa-a-aš-šū*-. . .] (KUB, XXVII, No. 36 iv(?) 7) and *wa-a-aš-šū*-u-ša-a-. . .] (*ibid.*, 1. 6).

Now that it has been shown that the double sibilant was at times misunderstood as a single voiceless sound, the writing *te-šub* finds a ready explanation. Its use in Nippur names to represent the theophorous element *-teššup* results, no doubt, from imperfect understanding of the matter by the native scribes. At Nuzi, although *-teššup* is often correctly written in personal names, the writing *-te-šup* was preferred by the Hurrian scribes who should have known better. Thus, after having been a defective writing, it probably became a convention, recommending itself by its economy. To Nuzi scribes, who were very lax in expressing doubled sounds in writing, this short cut must have been very welcome, for *-teššup* is the most common element in Nuzi personal names.

An examination of the material adduced as evidence shows quite clearly that Akkadian scribes often overlooked the factor of quantity. It has already been observed that, when the Hurrians doubled a consonant, quantity alone was phonemic to them. However, the phonetic result was twofold: quantity and voicelessness. While the Hurrians were conscious of the former, they were quite indifferent to the latter. Conversely it may be said of Akkadian listeners that they were always aware of the voiceless effect, but only occasionally did they show themselves aware of quantity, which was the real issue at stake.

Up to now the phonetic behavior of the sibilant represented as *š* by the Hurrians has shown itself to be identical with that of the stops. However, this perfect agreement is now sharply interrupted. Postvocalic stops when final seem to be generally voiced, although there are some exceptions. The sibilant in question is consistently voiceless under these circumstances. The Ch.B. names Muzum-ari and 'Awešmuze include the elements written *-mu-ze* and *mu-zu-um* based on the Hurrian root *muš*. When Hurrian *muš* occurs alone without the em-

bellishments of stem vowel or other formatives, the phonetic result is not *muz* or the like but *muš*, as in *mu-úš* (Mari 6:10, 11, 15, 19). What is more, final *š* is unanimously revealed in *al-lu-la-da-i[š]* (Mari 5:7), in *a-ni-iš* in the name *Aniš-hurbi* from a Mari king list,⁸⁴ in *aš-ti-ni-iš* (Mari 4:25), *a-wa-an-du-úš* (Mari 3:23, 26), and in *a-we-eš* in Ch.B. 'Aweš-muze discussed above. Other instances of final *š* are *e-ni-eš* (Mari 6:12, 21), *e-ni-iš* (Mari 1:32 and 2:12, 16), *e-ni-wu-úš* (Mari 6:10, 11, 18, 19), *hi-ia-ri-ia-aš* (Mari 4:28), *hi-in-zu-ru-úš* (Mari 6:7), *ke-ra-ri-ia-aš* (Mari 4:27), *i-su-di-iš* (Mari 5:6), *mu-ga-ri-iš* (Mari 6:15), *pa-ar-hi-wa-aš* (Mari 6:13), *ša-ma-ḥa-aš* (Mari 4:22), *ta-nu-úš* (Mari 6:17), *ú-né-eš* (in Ch.B. 'Uneš-na), and *ú-nu-úš* (in Ch.B. 'Unuš-kijaze, 'Unuš-šalli, and 'Unuš-umar).⁸⁵

Final *š* is phonetically so well entrenched that it has a strong retrogressive force. What is by now the well-known ending *-šuš*, a plural subject or agentive form,⁸⁶ should be something like *-zuš* at Mari. As such it is to be found in [. . .]'e-ḥu-na¹-zu-úš (Mari 6:14). But this instance seems exceptional in view of *pa-pa-na-šu-uš* (Mari 5:8) and *ši-we-na-šu-úš* (Mari 5:9), which apparently exemplify a retrogressive assimilative force of the final sound. Oddly enough this process happens to preserve a phonetic effect in keeping with the Hurrian phonemic system.

The sibilant, when final, results in *θ* in Ugarit writing, as e.g., in *Kmrbnθ* = *Kumarbiniš* (RS 4:8), *kldnθ* = *keldiniš* (RS 4:2), *enθ* = *eniš* (RS 4:39). However, difficulty is presented by *ḥzłz*, mentioned above, which has been compared with *ḥašulieš*. The form to be expected in Ugarit is *ḥzłθ*, like *pžłθ* (RS 4:36). According to the conclusions attained so far in this article, the actual Ugarit writing *ḥzłz* implies a form in which the final sibilant is followed by a vowel. While suggesting this hypothesis as a possibility, the writer does not insist upon it. First of all, all the evidence serving to demonstrate such grammatical variations of the *-lieš* formation is not available. Second, there may be involved phonetic processes which the writer, for one, is not at present competent to deal with. Hence any suggestions offered by him would be a matter of guesswork, which generally turns out to be more misleading than helpful.

⁸⁴ Occurring twice as *A-ni-iš-ḥu-ur-bi* (Syria, XX [1939], 109).

⁸⁵ Respectively written 'U-né-eš-na, 'U-nu-úš-ki-ia-ze, 'U-nu-úš-ša-li, and 'U-nu-úš-ú-mar.

⁸⁶ See Friedrich, *Kleine Beiträge zur churritischen Grammatik* (Leipzig, 1939), pp. 10-12.

III

Also subject to pattern was the phoneme variously expressed *p/w*,⁸⁷ for where correspondences are obvious the pattern makes itself felt when this sound is expressed by *p* or *b* by Semites. That true quantity is involved in the postposition *ww*, "my," finds confirmation in *pa-ḥi-ip-pi-ni-im* (Mari 1:32). That the accompanying effect of voicelessness ensued is attested by the writing *p* in Ugarit *atynpθ* (RS 4:3) and *atynpd* (RS 4:4), which must express underlying *attainippaš* and *attainippada*, "my father" and "to my father."⁸⁸ Elsewhere the pattern is followed by Hurrian *want*, which appears as *pand* in 'pa-an¹-di-en (Mari 6:19) and in Nippur *Pa-an-di-ia* (BE, XIV, No. 162:8, and XV, No. 199:6).⁸⁹ Similarly, Hurrian *waḥr* is expressed as *paḥr* in Ch.B. 'Paḥri-šehirni and 'Paḥri-uzuwe.⁹⁰ Apil-Sin, an Akkadian scribe at Nuzi,⁹¹ writes *Pa-[ar-ḥi-š]e-ni* (SMN 3101) for the common Nuzi name *Waḥri-šenni*. Also concerned are the words *pa-ar-ḥ[a- . . . -d]a* (Mari 3:21) and *pa-ar-ḥi-wa-aš* (Mari 6:13). The deity *Wišaišaphi* is represented as *Pžžpḥ* (RS 4:35, 37, and 50 obv. 5). The deity *Kumarwe*⁹² appears as *Kmrw* (RS 4:6, 7, 8, and 7:1 f., 8 f.). If *b* in *ḥmrbn* (RS 4:60) actually represents the genitive *-we*,⁹³ it does so under pattern conditions, for it is preceded by *r* or by a vowel.

Similarly concerned is the velar spirant *ḥ*, for it is again one of those phonemes which had voiced and voiceless phases unperceived by Hurrians but discernible to the ear of Semites. Again this phonetic distinction is governed by pattern. Such a state of affairs is revealed in Ugarit, where single intervocalic *ḥ*, under its voiced aspect, turns out to have been something like a *ghayin*, hereafter to be symbolized by *ḡ*, for it is under this guise that it appears in the alphabetic writing.⁹⁴

⁸⁷ See F. Bork, *Die Mitannisprache*, M V A G, XIV, Parts 1/2 (Berlin, 1909), 24, and Berkooz, NDA, p. 50, G.

⁸⁸ See von Brandenstein, ZDMG, XCI, 560, for translation "father."

⁸⁹ Also *Pa-an-di-ia* (SMN 3082, 3094, and 3101), written by Apil-Sin, an Akkadian scribe at Nuzi, on whom see "Early Scribes," p. 171.

⁹⁰ Written 'Pa-aḥ-ri-še-ḥi-ir-ni and 'Pa-aḥ-ri-ú-zu-we.

⁹¹ See "Early Scribes," p. 171.

⁹² So expressed by the writings ⁴*Ku-mar-w[e]* (KBo, V, 2 li 60), ⁴*Ku-mu-ur-we* (AASOR, XVI, 47:1; 48:1), ⁴*Ku-mar-we-ne-eš* (KUB, XXVII, 38 iv 21), and *Ku-ma-ar-wi-ni-da-al* (Mari 5:4).

⁹³ As suggested by von Brandenstein, ZDMG, XCI, 568.

⁹⁴ Sign No. 21 in Gordon's list, An. Or., XX (1940), 10, the reading of which is still in dispute. It was read *x* by Friedrich, An. Or., XII, 126 f., in the form *tθḥ ḥlḥḡ/x* = Hurrian

Thus *h̄lb̄g* (RS 4:10) seems to be identical with the Hurrian form *h̄alpahe*.⁹⁵ By the same token, *lbt̄g* (RS 4:36), as Speiser suggests,⁹⁶ should be *lubtuhe*. Ugarit *p̄gn* (RS 372 A 18) and *p̄gdm* (RS 4:3) are reminiscent of the stem *paḥi* occurring in Boğazköy texts.⁹⁷ With greater certainty *t̄ḡnnk* (RS 4:49) can be interpreted as a form arising out of Hurrian *teḥša*.⁹⁸ In the same line a stem based on *teḥ* is to be found in *t̄g*. The voicing of Hurrian *h* under the guise of *ḡ* beside *r* is demonstrated by the Ugarit treatment of Hurrian *nihr*, as in *n̄grn* (RS 4:53 and 372 A 11) and *n̄r̄gp* (RS 4:58), which reveals the metathesis apparent in *ni-ih-ri-ia* (KUB, XXVII, No. 34 iv 10) and *ni-ir-hi-ia-aš-ši* (KUB, XXVII, No. 34 iii 13).⁹⁹ In addition, *ʾagr* (RS 7:1, 8 and 34+35:1, 13) apparently expresses the root forming the basis of Hurrian *a-aḥ-ra-a-i* (KUB, XXVII, No. 23 iii 4) and *aḥrušhi*.¹⁰⁰ That *h* can be voiced when final is suggested by *ʾeyk̄z̄g* (RS 4:15), which suggests the phrase *eiḡa Kušuh* (the word-divider being omitted, as it often is in Ugarit texts).

Elsewhere, i.e., when initial and when doubled, Hurrian *h* appears as Ugarit *h* in examples too abundant to be cited here. Nevertheless, it is noteworthy that etymological evidence for quantitative *h̄h* may be sought in the Hurrian word for "female," occurring alternately as

Teššup Ḥalpaḥi, a comparison previously suggested by Hrozný in *AOr*, IV (1932), 128. Von Brandenstein treats it as sort of a sibilant. However, D. H. Baneth (*OLZ*, XXXV [1932], 705) and Ginsberg (*OLZ*, XXXVI [1933], 593 f.; Virolleaud, *Syria*, XIII [1932], 125, n. 1) read it as *ḡ*. Speiser (*JAOs*, LVIII, 197-201) read *ḡ* but later in *Language*, XVI, 334-36 became involved in uncertainties arising from method rather than from the evidence.

The Hurrian phoneme *h* in its voiceless aspect is practically the same as Semitic *h*. Under its voiced aspect it became something like *ḡ*, a change which was a matter of indifference to the Hurrians but which misled the Semitic listeners at Ugarit into thinking that an actual phonemic difference analogous to their own was involved. This misconception is not observable among Akkadian scribes who had to deal with Hurrian. In the first place, *ḡ* was not one of their phonemes. Second, the cuneiform syllabary provides no ready means which would have enabled them to express any phonetic difference they may have noticed between *h* and *ḡ*.

⁹⁵ See preceding note.

⁹⁶ In *Language*, XVI, 335.

⁹⁷ Cf. *pa-a-ḥi* (KUB, XXIX, 8 iv 11, 25), *ʾpa-aʾ-ḥi-laʾ-aʾ-e-na* (KUB, XXVII, 38 i 14), *pa-a-ḥi-ip* (KUB, XXIX, 8 iii 21), *pa-a-ḥi-pa* (KUB, XXIX, 8 ii 36), and *pa-a-ḥi-ta* (KUB, XXIX, 8 iii 9, 12, 18).

⁹⁸ See n. 62.

⁹⁹ *Nihr* occurs also in the Nuzi names *Nihrija*, *Nihri-teššup*. For an instance of the same metathesis as observable in *Nihri-teššup* and *Nirhi-teššup* see Berkooz, *NDA*, p. 64.

¹⁰⁰ Occurring frequently in Boğazköy texts. See A. Götze and H. Pedersen, *Muršili's Sprachlehre* (København, 1934), p. 31 and n. 1.

aštuḥena and *aštuḥḥena*.¹⁰¹ Outside of the fact that the former quite possibly is defectively written, the quantity in *h̄h* of *aštuḥḥena* in one instance can perhaps be accounted for by the ignored form *aš-tù-uš-ḥe-e-na* (KUB, XXVII, No. 3 i 12), which indicates original *šh* > *h̄h* by assimilation. That *š* has assimilative tendencies becomes evident when the Nuzi name *Kuš-ki-pa* is compared with its variant *Kuk-ki-pa*.¹⁰²

IV

The phonetic system which was termed as stop-pattern at the beginning of this article turns out to be a consonantal pattern that involves the stops *k*, *p*, and *t* and the spirants *h*, *š*, and *p/w*. Excluded from this scheme are the sonants *l*, *m*, *n*, and *r*. Whether another Hurrian sibilant, this time *s*, not *š*, is to be included cannot be affirmed or ruled out until a very serious study is undertaken. It is well known that in writing during the Old Babylonian period and even later, Akkadian *s* was expressed by *z*-containing signs. The same ambiguity characterizes the writing at Nuzi, as established by Akkadian scribes.¹⁰³ By them *s* was expressed by the signs *zi*, *za*, and *zu*. The sign *si* was used only with the value *ši*. Thus *Sin-banī*, another member of the small colony of Akkadian scribes, writes the Akkadian form *sasinni* as *sà-si-ni* (HSS, V, No. 65:16, wrongly copied, as discovered by Dr. Lacheman). Akkadian *kīsu*, "pouch," is written *ki-si* (SMN 3094:13) by Apil-Sin.¹⁰⁴ Ṭāb-milk(i)-abi writes *si-ki-il-ta* (HSS, V, No. 71:18) and *si-ra-šū-ū* (JEN 404:36) for Akkadian *sikilla*, which designates a type of property, and *sirašū*, "vintner." The Akkadian forms *ipussu* and *mārassu* appear, respectively, as *i-pu-sū* (HSS, V, No. 65:3) and *ma-ra-sū* (*ibid.*, 1. 5) written by Sin-banī.

The sign *si* when encountered in texts written by the Akkadian scribes at Nuzi has the value *ši*, as in *ši-mi* (SMN 3082:10), written by Apil-Sin; *i-ši-mu* (HSS, V, No. 71:4), *ši-im-ta* (*ibid.*, 1. 2), *ši-im-ti*, (*ibid.*, 1. 1), written by Ṭāb-milk(i)-abi. Both Amurru-šar-ilāni and

¹⁰¹ For examples, see von Brandenstein in *ZA* (N.F.), XII (1940), 113.

¹⁰² Cf. *Ku-šū-ki-pa* (JEN 396:13) and *Ku-uk-ki-pa* (JEN 537:3) with same genealogy.

¹⁰³ See "Early Scribes," p. 171, for evidence indicating relationship of Nuzi tablets to those prevalent in the Old Babylonian period.

¹⁰⁴ What Ṭāb-milk(i)-abi meant by writing *ʾgi-iaʾ* LUGAL.GAL (JEN 404:26 f.) is not clear.

Ṭāb-milk(i)-abi write *mar-šī-ti* (JEN 414:8 and HSS, V, No. 71:26) for Akkadian *maršīti*.

Since these scribes were Akkadian, they must have known what they were doing, and thus the all-embracing use of *z*-containing signs at Nuzi turns out to be a matter of orthography rather than the result of phonetic misunderstanding of Akkadian sibilants by the Hurrian populace. These rules apply to Nuzi orthography in the main, but there can be found exceptional instances in which *s* seems to be expressed by *s*-containing signs.¹⁰⁵ That this intrusive trend took root in the early scribal period is evident in *Ē-su* = *bīssu*, "his house" (JEN 570:20).¹⁰⁶

Nevertheless, it becomes obvious now that *z*-containing signs rather than so-called *s*-containing signs at Nuzi provide the medium par excellence for expressing *s*, a phoneme in Hurrian as well as in Akkadian. Confirmation of this state of affairs is amply provided in the Nippur documents, where, for the most part, *s*-containing signs rather than *z*-containing signs are employed to portray *s*. The actual character of the sibilant in the Nuzi name written *Ka₄-ni-iz-za*¹⁰⁷ is revealed as *s* by the Nippur scribes, who are the most reliable as far as portrayal of actual phonetic results is concerned. They write *Ka-ni-is-si* (CBS 3473 A 3), which imposes the reading *Ka₄-ni-is-sà* for the Nuzi writing. There are many Nuzi names in which suffixal formations involving *ss* are expressed exclusively by *z*-containing signs.¹⁰⁸ In Nippur this suffix is generally written with *s*. The pertinent examples are *A-gi-is-si* (BE, XV, No. 190 ii 31; PBS, II, Part 2, No. 11:7), *En-na-as-si* (CBS A3), *Ka-ni-is-si* (already cited), *Ki-ir-ma/ba-as-si* (CBS 3474), *Pa-pa-as-si* (CBS 3474), *Ta-gu-(us)-si* (CBS 3474). Additional comparisons of this sort change, e.g., Nuzi *Ki-iz-zu*¹⁰⁹ to *Ki-is-sú*,

¹⁰⁵ E.g., in the different writings of the name Partasua (see Berkooz, *NDA*, p. 62). All the same, the question of sibilants in Nuzi is to be reappraised, probably along the lines set forth by Goetze, *Language*, XIV (1938), 136 f., with regard to Nuzi, and recently in *Language*, XVII (1941), 128 f. and n. 15 and pp. 168 f., with regard to conventions in writing in the Amarna letters.

¹⁰⁶ Written by the unknown Akkadian member of the early scribal group at Nuzi (see "Early Scribes," p. 171).

¹⁰⁷ JEN 360:2, 7, 10 ff.

¹⁰⁸ For examples, see Oppenheim, *WZKM*, XLIV, 206.

¹⁰⁹ *AASOR*, XVI, 20:20.

*Pa-az-zi-ia*¹¹⁰ to *Pa-as-si-ia*, *Zi-iz-zi-ia*¹¹¹ to *Si-is-si-ia*, and *Zu-un-na*¹¹² to *Sú-un-na*, for their respective Nippur occurrences are written *Ki-is-si* (CBS 11683), '*Pa-as-si*', (BE, XIV, No. 112:15), *Si-is-si* (BE, XIV, No. 19:13, and XV, No. 198:5), and *Si-is-si-ia* (BE, XIV, No. 19:65) and *Su-un-na* (CBS 3480). As observed before,¹¹³ the Nuzi name ordinarily taken as *Zil-teššup* is rendered as *Si-il-te-šub* (PBS, II, Part 2, No. 84:41, and CBS 3480), indicating, of course, that the Hurrian root in question is *sil* throughout, but not *zil*. Likewise the Nuzi names previously understood by the writer and others to be *Zike*¹¹⁴ and *Zikipa*¹¹⁵ turn out to be actually *Sike* and *Sikipa*, for they appear in Nippur as *Si-ge* (PBS, II, Part 2, Nos. 13:45; 84:20, 39; 132:32; BE, XIV, No. 138:3, and XV, No. 198:25) and *Si-gi-ba* (CBS 11869). The name written '*A-zu-e*' (G 76:6) at Nuzi expresses underlying '*Assue*, a pronunciation revealed by '*As-su-me*' (PBS, II, Part 2, No. 111:3, and CBS 3638).

Obviously *z* whenever encountered in the Mari material and in the Hurrian names from Ch.B. is to be read circumspectly, since readings with *s* now are not only possible but probable. As already observed in Part II, *s* perhaps properly written with *si* makes its appearance in Ch.B. '*Ha-zi-ib-si-mi-ga*. In Mari properly written *s* occurs in *i-si* (Mari 5:1, 5), *i-su-di-iš* (Mari 5:6), and *ma-ru-sa* (Mari 5:15). That such usage is unusual is indicated by the fact that these writings occur in only one of the six texts concerned, and attest, therefore, the style of a particular scribe. Moreover, the system of writing which clearly distinguished between *s* and *z* or *š* did not become commonplace until the Kassite period. Being sparse and ambiguous, this evidence provides no reliable means for reaching final judgments on Hurrian *s* other than its phonemic existence in Hurrian. The proper time has not yet come to judge whether or not it became voiced as *z* under the appropriate phonetic conditions. What is known is the interchange of initial Hurrian *š* with it discussed in Part II of

¹¹⁰ JEN 5:28 *et passim*. Genealogy imposes the reading <Pa>-as-si-ia for JEN 418:28.

¹¹¹ JEN 482:19, 30. This applies, no doubt, to the city name written *Zi-iz-za*, or, better, *Si-is-sà*, in the Nuzi tablets and the identically written name element. Both must be *Sissa*.

¹¹² HSS, V, 16:22, 34.

¹¹³ "Early Scribes," p. 177, n. 66.

¹¹⁴ Generally written *Si-ke*(ar) at Nuzi, where it is very common (see, e.g., JEN 46:2, 10 ff.).

¹¹⁵ Various written *Si-ki-pa* and *Si-ki-pá* (JEN 636:5, 8, 9 ff.).

this article. The clue to this and other related problems no doubt lies in the Hurrian written in the alphabetic texts at Ugarit, where *s*, *z*, and perhaps *š* occur.¹¹⁶ However, comparisons of the words in which they occur with known Hurrian words are not at hand, and thus this very fruitful study must await a later date.

V

Despite lack of information about this interesting phoneme *s*, the stage must be set for a survey to determine the phonetic effect of Hurrian consonants when they are contiguous. When the pattern was first discovered, it was observed that stops were voiceless after Ugarit *θ*.¹¹⁷ But *θ* and its counterpart, *š*, in Hurrian written by Akkadian scribes, have been found in this article to represent the voiceless aspect of the sibilant represented by the Hurrians as *š*. The observation alluded to above concerning the voiceless stops after *θ* can now be stated in another way. That is, *when any two pattern consonants occur together, both are voiceless*.

Beginning with the Ugarit consonantal clusters involving *θ*, *θk* occurs in *θuθk* (RS 4:22, 49:2, 6, and 372 B 8) and in *θwθk* (RS 34+45:2, 12, 14) which have long since been identified with the name of the Hurrian goddess Šauška.¹¹⁸ The voiceless cluster *θt* is to be found in *ʔaθh[n]* (RS 4:55), *ʔaθhnm* (RS 4:56), and *ʔaθhnēr* (RS 4:58), all based on Hurrian *ašt*,¹¹⁹ in *ʔAθtb* (RS 4:29, 31) and *ʔAθtbd* (RS 50 rev. 3), representing the Hurrian deity Aštapi,¹²⁰ and perhaps in *tnθt* (RS 7:11-12), possibly representing the Hurrian verbal form **tanašta*.¹²¹ To be discounted perhaps is *gθhp* (RS 4:61), which has been tentatively associated with Hurrian *kešhi*.¹²²

¹¹⁶ See Speiser, *JAOS*, LVIII, 177, and von Brandenstein, *ZDMG*, XCI, 575 f.

¹¹⁷ Friedrich, *An. Or.*, XII, 131; von Brandenstein, *ZDMG*, XCI, 574.

¹¹⁸ By Hrozný, *AOr*, IV, 127, n. 1; H. Bauer, *OLZ*, XXXVII (1934), 475.

¹¹⁹ See von Brandenstein, *ZDMG*, XCI, 567 f., and ZA (NF), XII, 113.

¹²⁰ See Hrozný, *AOr*, IV, 123.

¹²¹ Cf. *ta-a-na-aš-to(du)-en* (Tuš. iv 15).

¹²² By von Brandenstein, *ZDMG*, XCI, 569. The initial voiced stop violates the consonantal pattern, while in the Ch.B. name element *-ke-ēš-he* in *ša-du-um-ke-ēš-he* the stop in question conforms to pattern. Under these circumstances it looks as if the *g* in the RS occurrence under study is actually a word-divider. Hence a word *šhp* is possibly at issue. Speiser (*Language*, XVI, 332) uses *gšhp* as evidence for aspiration of Hurrian stops. To this he couples von Brandenstein's assertion, unconfirmed by citation or reference, that *kešhi* occurs with initial *h* in an unpublished text from Boğazköy. Although the writer believes that aspirated stops are by no means impossible in Hurrian, he does not, for one, feel that the doubtful Ugarit *gšhp* and the purported but uncited form *hēšhi* constitute adequate evidence.

Similar evidence is yielded by Akkadian renditions of Hurrian, viz., Nippur *A-bu-uš-ki* (*BE*, XIV, Nos. 58:34, 60:17, 62:6), Ch.B. *A-re-ēš-ka-an*, *ʔAš-tu-a-la-na* (=ʔAštua-tanna), *ʔAš-tu-e*, and *ʔAš-tu-za-ar* (=ʔAštu-šar), and Nippur *Aš-tar-til-la* (CBS 3480) (=Aštar-tilla). Here the voicelessness of *šk* and *št* is beyond question. In regard to the latter the same is confirmed by *aš-ti-ni-iš* (Mari 4:25), *ha-za-aš-la-ri* (Mari 3:18, 19), *mi-iš-ta-ḥu-ūš* (Mari 6:4), *pt!-ši-iš-ti-di-en* (Mari 5:17).

Voiceless *š* occurs immediately adjacent to other consonants presumably voiceless also in [. . . -a] *h-šu-ūš-hi-ni-el* (Mari 4:23), *ak-šum* (Mari 3:26), *ku-šu-uk-še-en* (Mari 1:11), *aš-ḥu-u[n]* (Mari 6:14), *uš-ḥa-lu-ru-um* (Mari 4:26), [. . . -] *ti-ip-ša-ri* (Mari 6:7). In personal names the same occurs in Ch.B. *Na-wa-ar-ni-iš-he* (=Nawar-nišhe), *Ša-du-um-ke-eš-he*, and *ʔŠa-zu-um-ke-eš-he* (=Šatum-kešhe and perhaps *ʔŠašum-kešhe*, respectively).

Ch.B. *Ap-ša-am* may perhaps be included, but at present the writer is not competent to deal with it, for here the Hurrian sibilant *s*, not *š*, is concerned, the Hurrian root being *aps*.¹²³ Whether or not the change to *š* is a Hurrian matter, the root seems to be correctly understood in Ch.B. *ʔŠi-in-ap-ze/sē*. Incomplete knowledge of *s* postpones discussion of the problem.

The same effect is revealed in the case of Ugarit *ph*, a voiceless combination exemplified by *Pžžph* (RS 4:35, 37) and *Pžžphnd* (RS 50 obv. 5), which represent the deity Wišaišaphi.¹²⁴

Difference in voicing of consonants performing identical morphological functions under varying phonetic circumstances is well illustrated in RS 50. This document is concerned with offerings made to Hurrian deities and calls for expression of the dative concept. It is not surprising, therefore, to find that the Hurrian postposition *-ta*, "to," appears after the name of each deity. Since these god-names end in vowels, Hurrian *-ta* is expressed by *-d*, showing that the phonetic result was *-da*. In obverse, line 9, of this very text Hurrian *-ta* does

¹²³ As in *ap-[su]-u-ša* and *ap-su-ša-a-ul-la-ma-an* (Tuš. iv 63). But in Boğazköy *ap-ši-ur-ra* (*KUB*, XXVII, 42 obv. 30) and *ap-še-ne-we* (*KUB*, XXVII, 38 ii 22) are puzzling, implying perhaps a *s/š* variation or confusion on the part of Hittite scribes of the two Hurrian sibilants, respectively known as *s* and *š*.

¹²⁴ Friedrich (*An. Or.*, XII, 131) observed that *p* was voiceless beside *h* in the Ugarit occurrence of this divine name.

not occur after a vowel but after θ in $h\dot{d}n\theta t\ h\dot{d}l\theta[t]$, which in Hurrian is $h\dot{u}tena\dot{s}ta\ h\dot{u}te\dot{l}lurra\dot{s}ta$, "to the $h\dot{u}te$ - $h\dot{u}telure$ gods."¹²⁵ But here it is written t , not d , for the phonetic result was $-ta$. The suffixal formation $-tukku$ appears in $\dot{s}a-al-hu-du-uk-ku$ (Mari 6:8), where the dental is voiced as a result of its intervocalic position. In the next line it appears as $ak-tu-uk-ku$, in which it becomes unvoiced presumably along with the immediately preceding k . In his earlier article¹²⁶ the writer demonstrated the same sort of phenomenon in the vagaries of $-te$, the shortened form of the theophorous element $-te\dot{s}\dot{s}ub$ at Nippur. With the resultant loss of the pause between elements it became $-de$ under pattern conditions. But in the name $Il-hap/hip-te$ (CBS 4574), a shortened form of $*Ilhip-te\dot{s}\dot{s}ub$, it resumed its normal voiceless character. Presumably the preceding formative p did the same. The writer alluded also to the behavior of the suffixal formation $-tien$ ¹²⁷ as illustrated by $ha-tu-di-en$ (Mari 5:19) and $ki-ip-ti-en$ in the following line. Voiceless k after p , which presumably is also voiceless, occurs, incidentally, in Ch.B. $Na-wa-ar-tu-up-ke$ (=Nawar-tupke) and $Tu-up-ki-ta-na$ (=Tupki-tanna).

With regard to the matter of contiguous voiceless consonants, certain deviations may be possible, as in $lbt\dot{g}$ (RS 4:36), which smacks very much of Hurrian $luptu\dot{h}e$, "the one of Lupti." Nevertheless, in the majority of instances in which two consonants are encountered in the Ugarit texts, both consonants are represented as voiceless. The testimony from Akkadian scribes provides quite complementary evidence to support this view. Obviously this new law concerning the voicelessness of contiguous consonants is identical with the one concerning the voicelessness of double consonants which merely reflects instances in which contiguous consonants happen to be similar. Thus the consonantal pattern can be described very simply as one in which consonants are voiceless when initial and contiguous, but voiced when occurring singly after vowels and adjacent to sonants. With the exception of the sibilant known as \dot{s} , they have an inclination to become voiced when final. A much simpler way of describing the situation is to amend the observation Bork made on stops long ago¹²⁸ and apply it

¹²⁵ This point was noted by von Brandenstein, *ZDMG*, XCI, 574.

¹²⁶ "Early Scribes," p. 179.

¹²⁷ *Ibid.*, p. 185, n. 112.

¹²⁸ *Die Mitannisprache*, p. 9.

to Hurrian consonants in general, for voicing of consonants had nothing to do with the Hurrian phonemic system. To all intents and purposes they were essentially voiceless. As already observed,¹²⁹ the Hurrians seem to have pictured all Akkadian stops as voiceless. Accordingly, unless aspiration is to be considered, their system phonemically had only one set of consonants. Under this guise the Hurrian consonantal system is not unusual, for it finds parallels in other languages.¹³⁰

VI

By way of conclusion the writer departs from the field of fact to that of hypothesis in an attempt to explain the relationship between Hurrian and Hittite writing. Since the Hurrians were geographically situated between Hittite Asia Minor and Akkadian Mesopotamia, which was the source of culture, they presumably may have provided a channel through which Akkadian culture was transmitted to Hatti. Accordingly, it may very well have been through them that the Akkadian syllabary came to the Hittites.

If this is what happened, it can be assumed that distinction between voiced and voiceless consonants, customary in Indo-European languages, was felt also in Hittite. The Hurrian consonantal system being what it was, the syllabary would have been one in which this distinction was ignored in the script. For instance, the signs GA, KA, and QA would all three have been represented to the Hittites as having essen-

¹²⁹ As in the case of the Nuzi scribe Atal-teššup (see "Early Scribes," p. 185).

¹³⁰ Dr. Sachs recalled to the writer's attention Sapir's important article, "Sound Patterns in Language," *Language*, I (1925), 37-51, in which among the items studied there is revealed a situation quite comparable to the one demonstrated in the present article. This involves, among other matters, Upper Chinook, a language characterized by consonants which are primarily either voiceless or aspirated. Voicing of the unaspirated consonants occurred under practically the same pattern conditions as in Hurrian without being noticed by the speakers of the language concerned (see *ibid.*, pp. 42-44.). The same phenomenon becomes quite obvious to one who reads L. Bloomfield's article "On the Sound-System of Central Algonquin," *Language*, I (1925), 130-56. See also Bloomfield's *Language* (New York, 1933), pp. 82 f. That the consonantal pattern of the Hurrian type is no uncommon phenomenon is attested by its appearance in Dravidian languages (see G. W. Brown, *JAOS* L [1930], 279 f.), where it appears to be a modern development; see, e.g., Speiser in *Language*, XVI (1940), 339, n. 59, who refers to "Linguistic Survey of India IV," p. 288. The writer finds these parallels most welcome; they suggest that Hurrian too may have had aspirated as well as voiceless stops. As in the Dravidian languages, the stop-pattern may very well have been a transitory phase in the development of Hurrian and related languages. Naturally, the consonantal pattern must be considered as a sporadically occurring linguistic development rather than as evidence of genetic relationship of the languages in which it is observable.

tially the value *ka*. In addition, the Hittites, speaking an Indo-European language, may have readily perceived that these three signs had the value *ga* when following vowels. This would have permitted them to use these signs ambiguously in this regard. When *ga*, *ka*, and *qa* occurred after signs ending in *k*, resulting in intervocalic double *k*, the Indo-European Hittites, like the Semites, probably understood the effect as a single voiceless sound and not always as the intended lengthened sound. Accordingly, they would have been misled to believe that voiceless consonants had to be written double as digraphs. The Akkadian syllabary as utilized by the Hittites for expressing their language conforms to this hypothesis. As is well known, distinction in voice is an equivocal issue as far as the individual signs are concerned. However, as has been observed, many instances of double writings of stops and perhaps of other consonants in Hittite correspond quite regularly to single Indo-European voiceless sounds.¹³¹

Since this hypothesis is based only on inferential grounds, it stands in need of documentary proof, which may or may not come to light. Moreover, it is also quite possible that others may demonstrate it to be incorrect. Therefore the writer, who has an aversion toward so-called "predictions," presents it only as an idea which he strongly believes in for the present. More important than this theory is the factually demonstrable Hurrian consonantal pattern, which is the immediate purport of this paper and which the writer places at the service of those engaged in Hurrian studies.¹³²

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UNIVERSITY OF CHICAGO

¹³¹ For a summary of the problem, see E. H. Sturtevant, *A Comparative Grammar of the Hittite Language* (Philadelphia, 1933), pp. 74-86, and his confirmatory evidence in *Language*, XVI (1940), 81-87. See also nn. 60 and 61 in the present article, where it is believed that *š* in Hurrian writing might even express a sibilant with *s* and *z* as the respective non-phonemic phases.

¹³² The conclusions reached in the foregoing article conflict with certain views expounded by Professor E. A. Speiser, in *Language*, XVI, 319-40, in an article dedicated to the thesis that double writings in Hurrian texts were digraphs expressing single voiceless consonants. The writer, on the other hand, holds that, since difference in voice was not phonemic in Hurrian, double consonants in Hurrian writing were simply a direct expression of double consonants in speech and hence voiceless like any other Hurrian consonantal cluster. The reader is advised to read Professor Speiser's article to see the other side of the question.

THE ORIENTAL INSTITUTE ARCHEOLOGICAL REPORT ON THE NEAR EAST, 1941

Compiled by

GEORGE R. HUGHES (Egypt and Nubia)
JOSEPH P. FREE (Palestine, Transjordan, Syria, Cyprus)
WALDO H. DUBBERSTEIN (Turkey, Iraq, Iran, India, U.S.S.R.)

EGYPT AND NUBIA

Abusir (in the Delta). Department of Antiquities

Excavations and restorations begun in 1937 (*AJSL*, LV, No. 4 [October, 1938], 426) on the ancient sites of Plinthiné and Taposiris, 40 kilometers west of Alexandria, were continued in 1940. The great tower, octagonal at the bottom and cylindrical at the top, has been completely restored from near ruin. The stairway in the octagon and the spiral staircase in the top are now rebuilt and show the means of access to the summit. Once thought to be a funerary monument, the tower seems clearly to have been a lighthouse. It is the only building of its kind extant in Egypt and may indicate what the great Pharos of Alexandria was like. The cemetery around it has proven on excavation to be of earlier date than the tower.

The excavation of the so-called "Temple of Osiris" was also continued. The building now appears to have been something like a convent with small cells about a central chapel. Pottery found under the floor of one cell points to a date at the earliest Hellenistic for the founding of the building.

From a Department of Antiquities release.

Alexandria. Alexandria Graeco-Roman Museum

In March, 1941, Alan Rowe discovered the richly ornamented body of a young woman in the Alexandria catacombs. The skull contained the gold tongue and eyes inserted at burial, and on the body were various rosettes, two necklaces, and fingernail coverings, all of gold. The catacombs, first discovered in 1900, date from the second century of our era.

New York Times, March 30, 1941.

Antinoë (Sheikh Abâdeh). Royal University of Florence

In May, 1940, the expedition continued soundings in the mound west of the northern cemetery, and further explored the temple of Ramses II. The soundings were in rubbish, hence no buildings were unearthed; objects of domestic use were, however, numerous. Many fragments and several small rolls of

papyrus were recovered. The late cemetery in the natural sand under the mound showed only very poor burials containing no coffins.

The excavation of the Ramses temple continued on the north and south sides and to the back, where the sanctuary must have been. The temple appears to have had a crypt added later. The entrance was in the angle of the south tower of the pylon. The crypt went down about 7 meters and consisted of a narrow passage, now below water level, which ran out in front of the temple under the court.

Some of the stone and mud-brick walls of a private house of Christian times now stand in the sanctuary area. Only here and there did the traces of walls and pavements of older buildings appear. However, these traces furnished more reused blocks bearing the cartouches of Ikhnaton (cf. *AJSL*, LVI, No. 4 [October, 1939], 423). The titles of the Aton are of the earlier type, showing that the original building was built early in Ikhnaton's reign. One door jamb has been reconstructed. The area also yielded several figures of deities and kings from Ramses II's time.

From a Department of Antiquities release.

(Edfu). *L'Institut Français d'Archéologie Orientale and University of Warsaw*

K. Michalowski et al., *Tell Edfou 1938* ("Fouilles Franco-Polonoises, Rapports," Tome II, Fasc. 2 [Le Caire, 1939]). This section is by J. Sainte Fare Garnot on the Pharaonic necropolis.

Emile Chassinat, *Le Mammisi d'Edfou* ("Mémoires ... de l'Institut Français ...," Tome XVI, Fasc. 2 [Le Caire, 1939]).

(Maasara). *Egyptian Museum in Stockholm*

In March, 1937, the Swedish expedition which had been working at Abū Ghalib opened six tombs at this site 20 kilometers south of Cairo. Five of these shaft graves had been plundered, two of them recently, but the broken pottery recovered dates them to the beginning of the Old Kingdom. The sixth, a pit grave mud-brick lined and stone covered, was intact. It furnished numerous fine examples of red and gray pottery and a few alabaster and slate bowls. These are definitively published by the excavator.

Hjalmar Larsen, "Tomb Six at Maasara: An Egyptian Second Dynasty Tomb," *Acta archaeologica*, XI, Fasc. 1-2 (1940), 103-24.

Nekheb (el-Kāb). *La Fondation Egyptologique Reine Elisabeth*

Jean Capart, "Un dépôt de fondation sous le sanctuaire," *Chronique d'Egypte*, No. 30 (July, 1940), pp. 205-10. The deposit is that of the last sanctuary of Nekhbet. The vases compare only with Achaemenid pottery found at Susa. The restorer of the sanctuary must have been a king of the Persian dynasty.

(Sakkarah). *Department of Antiquities*

Work was continued in 1939-40 in the area between the Step Pyramid and the pyramid of Unis. The clearing of the causeway of Unis resulted in the finding of blocks bearing interesting reliefs, including a famine scene. Mastaba

bas have been uncovered to the south of the causeway and others near that of Idut south of the Zoser inclosure.

In the burial chamber of one mud-brick mastaba south of the causeway was found a very unusual false door of acacia measuring 2 meters high by 1.50 meters wide. The mastaba is older than the causeway, for the building of the latter resulted in its being covered up. The burial chamber was otherwise empty, and the other rooms appeared inferior in workmanship. The false door was excellently preserved and has been moved to the Cairo Museum. The deeply cut hieroglyphs and the figures in low relief present the deceased Ika, a high functionary, his wife, Imerit, and twelve other members of his family.

Close to the funerary temple of Unis the excavators came upon the mastaba of a Queen Nebet, whom they believe to have been the wife of Unis. The rooms are large and the stone is of the best quality from the Tura quarries. The whole is richly sculptured and the figures and inscriptions are on an unusually large scale.

Another complete mastaba proved to belong to a vizier Mehu who lived under the first three kings of the Sixth Dynasty. Unpublished evidence is said to prove that Mehu was the grandson of Unis. The inscriptions show that he was married to two royal ladies. He must have been a great landowner, for forty estates are represented as contributing to his offerings. The entire tomb is in perfect preservation, even to the colors on the walls. The scenes present some hitherto unfamiliar subject matter in addition to the more common. One of the false doors is a single piece of limestone 3.1 meters high by 2.05 meters wide. It is painted dark red, as are the limestone roofs of the chambers, in imitation of red granite. The incised figures and hieroglyphs on it are painted yellow to simulate gold.

ILN, September 28, 1940, pp. 412 f.; *Chronique d'Egypte*, No. 30 (July, 1940), pp. 211 f.

(Sakkarah)

Lucienne Epron, *Le Tombeau de Ti* ("Mémoires ... de l'Institut Français ...," Tome LXV, Fasc. 1 [Le Caire, 1939]).

(Semneh)

Hermann Grapow, "Die Inschrift der Königin Katimala am Tempel von Semne," *ZAS*, LXXVI (1940), 24-41 and Pls. II, III.

Tanis (Šan el-Hagar). *La Mission Française de Tanis*

On the tombs of Kings Psusennes and Amenemope previously reported see also *JEA*, XXVI (February, 1941), 162; *Chronique d'Egypte*, No. 30 (July, 1940), pp. 212-14; *Scientific American*, January, 1941, p. 27.

Thebes (Deir el-Medīneh). *L'Institut Français d'Archéologie Orientale du Caire*

Clearance of the area north and east of the Ptolemaic inclosure wall added other small chapels dedicated by cemetery workmen to the forty or so previously found (cf. *AJSL*, LVI, No. 4 [October, 1939], 427). The most imposing to the north were one dedicated under Seti I to Amon and Hathor and another

dedicated to the deified Amenhotep I and Nefretiri. To the east was a large chapel of the reign of Ramses II. Among the chapels were found parts of the large statues which once adorned them as well as fragments of steles, offering tables, ostraca, papyri, and other objects.

JEA, XXVI (February, 1941), 162. Publications of previous work: Bernard Bruyère, *Rapport sur les fouilles de Deir el Médineh (1934-1935)*, Troisième partie: *Le Village, les décharges publiques, la station de repos du col de la vallée des rois* ("Fouilles de l'Institut Français ... du Caire," Tome XVI [Le Caire, 1939]); Jaroslav Černý, *Catalogue des ostraca hiératiques non littéraires de Deir el Médineh (Nos. 242 à 339)*, Tome IV ("Documents de fouilles de l'Institut Français ... du Caire," Tome VI [Le Caire, 1939]).

Thebes (Karnak). L'Institut Français d'Archéologie Orientale du Caire

Having relinquished the Madamūd concession, the French Institute began excavation of the temple of Montu at Karnak. Traces have been found of a temple dedicated by Amenhotep III to Amon. Under the pavement appeared blocks deriving from a square-pillared monument of Amenhotep II and others bearing geographical inscriptions from a temple of Amenhotep I. Important finds included a seated statue of Amon holding a kneeling figure of Amenhotep III in the costume of the *Sed* festival, and part of a stele of Haremhab dealing with the restoration of Theban temples. Montu does not appear until much later times, commonly on Ptolemaic blocks. Among these latter was one bearing a relief of the sacred bull of Montu.

JEA, XXVI (February, 1941), 161 f.

Thebes (Medinet Habu). Oriental Institute, University of Chicago

Uvo Hölscher, *The Excavation of Medinet Habu*, Vol. III: *The Mortuary Temple of Ramses III, Part I* ("OIP," Vol. LIV [Chicago, 1941]); Uvo Hölscher, "Gessodekorationen, Intarsien und Kachelbekleidungen in Medinet Habu," *ZAS*, LXXXVI (1940), 41-45.

Thebes (Sheikh Abd el-Qurna)

Nina M. and N. de G. Davies, "The Tomb of Amenmosé (No. 89) at Thebes," *JEA*, XXVI (February, 1941), 131-36 and Pls. XXII-XXV.

Thebes (Qurnat Murr'ai). Department of Antiquities

Under the direction of Baraize the tomb of Amenhotep III's well-known viceroy of Nubia, Mermose, was cleared. In it was discovered an anthropoid coffin on its sledge, both of granite.

JEA, XXVI (February, 1941), 162.

PALESTINE

General

Grace M. Crowfoot, "Some Censer Types from Palestine," *PEQ*, October, 1940, pp. 150-53.

Bethel (Beitin)

W. Ross questions whether Beitin is the Bethel of Jeroboam. He acknowledges that it is undoubtedly the Bethel of Jacob's dream but points out that another Bethel is mentioned in the Bible (Bethuel) and that there might be several Bethels. See *PEQ*, January, 1941, pp. 22-27.

Beth Shan (Beisân)

Alan Rowe, *The Four Canaanite Temples of Beth-shan, Part I: The Temples and Cult Objects* (Philadelphia, 1940).

The Palestine Archaeological Museum has recently purchased a small bone tablet, found on the east side of Beisan, representing a seven-branched candlestick with four other Jewish symbols. See *BJPES*, Vol. VIII, No. 1 (1940) (English summary).

Beth She'arim (Sheikh Abreiq). Jewish Palestine Exploration Society

For a résumé of the fourth season see Nelson Glueck in *AJA*, XLV, No. 1 (January-March, 1941), 116. According to Benjamin Maisler, it has now been possible to distinguish at Sheikh Abreiq four separate periods of occupation, extending from the first to the fourth centuries A.D. Also see *Palestine Review*, August 2, 1940, p. 71; *AJSL*, LVII (1940), 190, 323.

Jerusalem

R. W. Hamilton reports on three soundings in the area of the North Wall. Sounding A, at the west tower of the Damascus Gate, revealed two building periods, the second falling between the foundation of Aelia Capitolina and a probable repair of the curtain wall in or before the fourth century A.D. Sounding B, just to the east of Herod's Gate, confirmed the sequence of pottery types observed in A and also repeated the evidence for a construction in the third or early fourth century. The pottery also indicated a reconstruction of the wall in the sixth or seventh century A.D. Sounding C, just to the west of Herod's Gate, revealed two distinct lines of fortifications at this point, one represented by the present wall and its projecting tower, and the other by a rock scarp, which still carries the remains of masonry superstructures some 9 meters farther north. As at B, there is no evidence of a wall that could have belonged to any city earlier than Aelia Capitolina.

See R. W. Hamilton, "Excavations against the North Wall of Jerusalem, 1937-38," *QDAP*, X, No. 1 (1940), 1-54.

In August, 1940, a few large building stones were discovered during the construction of a new road just behind the property of the American School of Oriental Research in Jerusalem. Since these stones were located on a line directly east of the easternmost part of the Third Wall of Jerusalem which had been previously traced, the importance of the find was immediately evident, and the late C. S. Fisher and E. L. Sukenik immediately undertook a joint excavation in this area. A new stretch of city wall, 23 meters long and 4 meters wide, together with a tower about 12 meters long and 9 meters wide, was exposed. Some 140 meters farther east another tower, about 20 by 8 meters, has been discovered, thus extending the total explored length of the Agrippan Wall to nearly 800 meters.

W. F. Albright, "New Light on the Walls of Jerusalem in the New Testament Age," *BASOR*, No. 81, p. 10, and No. 83 (in press). Also Glueck in *AJA*, XLV, No. 1 (January-March, 1941), 116.

Kinnereth

Nelson Glueck, in *AJA*, XLV, No. 1 (January-March, 1941), 116, reports that in July, 1940, Benjamin Maisler, on behalf of the Jewish Palestine Exploration Society, undertook

a small excavation at Kinnereth, close to Khirbet Kerak on the Lake of Galilee. A grave was found containing seventy unbroken vessels, hundreds of beads, and ornaments of gold, bronze, and semiprecious stones. Maisler assigns the pottery to the middle of the Early Bronze Age, that is, to about 2500 B.C.

Lachish (Tell ed-Duweir). Wellcome-Marston Archaeological Research Expedition to the Near East

Lachish II (Tell ed-Duweir) The Fosse Temple, by Olga Tufnell, Charles H. Inge, and Lankester Harding (1940). D. Winton Thomas in *PEQ*, October, 1940, pp. 148-49, questions whether Ostrakon IV actually demonstrates that Tell ed-Duweir is Lachish. A series of philological notes on the epigraphic material discovered at Tell ed-Duweir and its bearing on the Bible is given by D. S. Mahbub in *Bible Documents' Notes I-IV* (London, 1939). David Diringer published some known and unknown inscriptions from Lachish in *PEQ*, April, 1941, pp. 38-56. Also see H. L. Ginsberg, "Lachish Ostraca New and Old," *BASOR*, No. 80, pp. 10-13; W. F. Albright, "The Lachish Letters after Five Years," *BASOR*, No. 82, pp. 18-24.

Lydda

BJPES, Vol. VIII, No. 1 (1940) (English summary) reports the finding, on the north-western outskirts of the ancient site of Lydda, of a small tombstone bearing what is believed to be a Samaritan inscription.

Megiddo (Tell el-Mutesellim). Oriental Institute, University of Chicago

J. W. Crowfoot in *PEQ*, October, 1940, pp. 132-47, suggests that the stables of Stratum IV belong to Ahab and not to Solomon. He bases his argument on the similarity of the masonry to that of the Omri and Ahab palaces at Samaria and also on the chronological sequence of the strata, which seem to fit more smoothly into the historical background if the stables are put in the period of Ahab. He gives a revised chronology for Strata III, IV, and V.

Mizpah (Tell en-Nasbeh). Palestine Institute of Archaeology of the Pacific School of Religion

Joseph Carson Wampler, "Three Cistern Groups from Tell en-Nasbeh," *BASOR*, No. 82, pp. 25-43.

(Nablus)

W. R. Taylor, "A New Samaritan Inscription," *BASOR*, No. 81, pp. 1-6. This inscription, exposed after heavy rains in 1935 on the main road at Nablus, is assigned to the third-fourth century and furnishes assistance in determining the chronological relations of other early Samaritan inscriptions.

(Tell ej-Jerisheh). Hebrew University in Jerusalem

J. Ory, "A Late Bronze Age Tomb at Tell Jerishe," *QDAP*, X, No. 1 (1940), 55-56. This is the first instance of a LB burial at this site.

TRANSJORDAN

Ezion-Geber (Tell el-Kheleifeh). American School of Oriental Research in Jerusalem

The excavation of March-May, 1940, showed further details of the smelting technique at this site. The smelter walls were probably 12 feet high, made of bricks laid in complex diagonal cross-patterns. The city itself was protected by two separate walls, with a glacis built against each of them and a dry moat between. The excavator assigns Period I to the tenth and ninth centuries and its most important phase to the time of King Solomon. Period II is assigned

to the ninth century, probably to the reconstruction by Jehoshaphat of Judah. Period III is assigned to the eighth century, when it may have been reconstructed by Uzziah. Period IV was Edomite, extending from the end of the eighth century B.C. to the end of the sixth. Not much is left of Period V, which lasted from the end of the sixth century to the fourth century B.C. Aramaic ostraca were found in this period. Glueck concludes that the cultural patterns of Ezion-Geber fit in with East Palestine rather than with West Palestine.

Nelson Glueck, "The Third Season at Tell el-Kheleifeh," *BASOR*, No. 79, pp. 2-18. Aug. Bea, in "Archäologische Beiträge zur israelitisch-jüdischen Geschichte," *Biblica*, XXI, Fasc. 4, 437-45, discusses the excavations at Ezion-geber and the question of its being the biblical site by that name. See also Glueck, "Ezion-Geber: Elah—City of Bricks with Straw," *Biblical Archaeologist*, III, No. 4 (1940), 51-55; "Ostraca from Elath," *BASOR*, No. 80, pp. 3-10, No. 82, pp. 3-11; and "Ezion-Geber: 'Singapore' of Solomon," *Asia*, December, 1940, pp. 663-69; W. F. Albright, "Ostrakon No. 6043 from Ezion-geber," *BASOR*, No. 82, pp. 11-15; Charles C. Torrey, "On the Ostraca from Elath," *BASOR*, No. 82, pp. 15-16.

Petra

M. A. Murray and J. C. Ellis, *A Street in Petra* (London, 1940): an account of the excavation of several caves.

(Teleilat Ghassul). Pontifical Biblical Institute

R. Koepfel, *Teleilat Ghassul II: Compte rendu des fouilles de l'Institut Biblique Pontifical 1931-1936* (Rome, 1940).

SYRIA

General

Jean Sauvaget, "Caravansérails syriens du moyen-âge," *Ars Islamica*, VII, Part I (1940), 1-19: Arabic inscriptions from a number of Mameluke caravansaries throughout Palestine and Syria.

Antioch. Princeton University, Baltimore Museum of Art, Worcester Art Museum, and Musées Nationaux de France

W. A. Campbell, "The Sixth Season of Excavations at Antioch-on-the-Orontes: 1937," *AJA*, XLIV (1940), 417-27: an extract concerning the development of the main street of Antioch, followed by a description of several mosaics. Also see Christine Alexander, "A Mosaic from Antioch," *BMAA*, XXXV, No. 12 (1940), 244-47.

Dura-Europus (Sālihīyah). Yale University and Académie des Inscriptions et Belles-Lettres

Clark Hopkins, "The Architectural Background in the Paintings at Dura-Europus," *AJA*, XLV, No. 1 (January-March, 1941), 18-29, compares the background of the Dura-Europus paintings with Roman and Hellenistic patterns and concludes that the evidence makes it probable that the Hellenistic Greek motives were the dominant element.

Hamath (Hama). Ny Carlsberg Foundation of Denmark

H. Ingholt, *Rapport préliminaire sur sept campagnes de fouilles à Hama en Syrie (1932-1938)* (Copenhagen, 1940); see also *Asia*, April, 1941, pp. 199-204.

(Jebel Kosseir)

C. W. McEwan, "Modelled Pot-Fragments from Jebel Kosseir, Syria," *Man*, XL (November, 1940), 167-69 (No. 193). Also see M. E. L. Mallowan, "Note on Modelled Pot-Fragments from Jebel Kosseir," *Man*, XL (1940), 169 (No. 194); Mallowan believes the scene represents animals led to sacrifice, as on the royal standard at Ur. He dates the fragments about 2500-2000 B.C.

Lebanon

Nelson Glueck in *AJA*, XLV, No. 1 (January-March, 1941), 117, reports that an expedition headed by Rev. Joseph Doherty, S.J., has completed the excavation of a prehistoric site in the Valley of Antelias in Lebanon. A petrified skeleton and the well-preserved skull of a child seven or eight years old, together with Stone Age tools and weapons, were found.

Ras-el-Ain. Marriner Memorial Syrian Expedition: Oriental Institute and Boston Museum of Fine Arts

C. W. McEwan, field director, will briefly describe the 1940 season at Tell Fakhariyya in the forthcoming number of this *Journal*.

(Sakçe Gözü)

Elaine Tankard, "The Sculptures of Sakjegeuzi," *AAA*, XXVI, Nos. 3-4 (December, 1939 [published July, 1940]), 85-88. A reinterpretation of the sculptures published in *AAA*, XXI (1934), 37 ff. The writer points out that the artist probably did not carve an oblique view of the face of the king but carved in the usual manner, working in two planes which are at right angles to each other.

(Tell Atchana). British Museum

Sidney Smith, "Timber and Brick or Masonry Construction," *PEQ*, January, 1941, pp. 5-10.

Ugarit (Ras Shamra). Académie des Inscriptions et Belles-Lettres

C. F. A. Schaeffer, *The Cuneiform Texts of Ras-Shamra—Ugarit* ("Schweich Lectures" [1939]); also *Cylindres de Ras Shamra—Ugarit* ("Première série" (Paris, 1940)). Schaeffer gives a description of the defense works and war machines of ancient Ugarit, based on the excavations, in *ILN*, June 14, 1941, pp. 778-80. Also see W. F. Albright, "Two Letters from Ugarit (Ras Shamra)," *BASOR*, No. 82, pp. 43-49.

CYPRUS

G. Ernest Wright, "The Syro-Palestinian Jar from Vounous, Cyprus," *PEQ*, October 1940, pp. 154-57. Wright dates the jar between 2700 and 2500 B.C. For earlier report see *PEQ*, July, 1939, pp. 162-68, where J. R. Stewart originally published the vase.

James A. Stewart, "Three Jugs of the Cypriote Iron Age in the Biblical Museum, Melbourne," *Man*, XL (October, 1940), 145 and 169 and Pl. K, dates this type of Cypriote pottery to Cypro-Archaic I, in the eighth-seventh centuries B.C. Also see John L. Myres, in *Man*, XL (November, 1940), 169-70.

John Franklin Daniel, "Prolegomena to the Cypro-Minoan Script," *AJA*, XLV, No. 2 (1941), 249-82.

TURKEY

General

Kurt Bittel has brief reports on recent small finds of Hittite materials and some unpublished museum objects in *Arch. Anz.*, LV (1940), 575-81, and also a summary of recently found monuments from the Greek and Roman periods (included are a relief of

Jupiter Dolichenus and a damaged stele depicting a warrior in the "late Hittite" style), in *ibid.*, pp. 584-88. Georg Rohde reports on another Jupiter Dolichenus stele dated to the second or third century A.D. in *ibid.*, pp. 596-99. Also see Kurt Bittel, "Der Depotfund von Soloi-Pompeopolis," *ZA*, XLVI (1940), 183-205; Albrecht Goetze, *Kizsuwatna and the Problem of Hittite Geography* ("Yale Oriental Series," *Researches*, Vol. XXII [New Haven, 1940]); P. Jacobsthal and A. H. M. Jones, "A Silver Find from South-west Asia Minor," *Journal of Roman Studies*, XXX, Part I (1940), 16-31.

(Alaca Hüyük). Turkish Historical Society

A beautifully illustrated report on the work carried on at this site has been made by Hamit Zübeyr Koşay. During 1939 the expedition concerned itself with further clearing in the Hittite temple area and with work in the lower levels of the Copper Age. The first cuneiform tablet found at the site was recovered in 1939. Two new tombs are reported from the Copper Age levels. A fine long dagger with a golden hilt was found in this area. Two fine statuettes with inset eyes are reported: one of copper and one of silver and gold. The small objects include rich decorative materials which seem to be related to Mesopotamian types but also have parallels in the Kurgans of South Russia.

La Turquie Kemaliste, Nos. 32-40 (August, 1939-December, 1940), pp. 20-26; *Arch. Anz.*, LV, 556 f.

Ankara

A brief survey of recent archeological work in and about Ankara by Hamit Zübeyr Koşay, "The Strata of Civilizations in Ankara," *La Turquie Kemaliste*, No. 31 (June, 1939), pp. 13-16.

(Bozkır)

At this site in a cave in the midst of the Taurus region Gaffar Totaysalgır discovered most unusual rock reliefs. One scene portrays a horseman with a long spear over a prostrate warrior. Another relief shows six rows of animals, apparently ibexes, and below them a dog. The style is reminiscent of African rock reliefs. There is no comparative material in Asia Minor, and no date may be assigned.

Kurt Bittel, *Arch. Anz.*, LV, 559.

Byzantium (Istanbul). Byzantine Institute

The Byzantine Institute carried on its work as usual from May to the end of December, 1940, in Hagia Sophia in Istanbul. Two life-sized figures were found, uncovered, conserved, and partly cleaned on the walkway of the north Tympanum wall. One of these figures represents Ignatius of Antioch and the other St. John Chrysostom. In the meantime the figures of the Mother of God and the Child in the eastern apse were opened to the public.

Excerpt from letter by Thomas Whittemore, July 28, 1941. See also illustrated article by Sami Boyar, "Aya Sophia," *La Turquie Kemaliste*, No. 41 (February, 1941), pp. 13-21; and A. M. Schneider, *Arch. Anz.*, LV, 589-91.

(Cağdırın)

At Cağdırın near the Sacur River a relief has been reported. The representation is presumably that of a god with pointed cap and sword and spear. Four damaged Hittite hieroglyphic signs appear above the figure, which is dated to the eleventh or tenth century B.C.

Arch. Anz., LV, 566 f.

(Çakalsuyu)

In the Çakalsuyu Valley in Malatya, Şevket Aziz Kansu reports the discovery of a Mousterian type of stone implement.

Arch. Anz., LV, 555 f.

(Ergazi)

At this site, a few miles southwest of Ankara, E. Pittard and Miss Afet report the discovery of about twenty stone implements of a Levallois-Mousterian or a Levallois type.

Arch. Anz., LV, 555 f.

(Göllüdağ)

The excavation carried out at this site under the direction of Remzi Oğuz Arık in 1934 was briefly reported in *AJSL*, LII (1936), 136, and LV (1938), 438. Now, for a concise review and discussion with photographs, see Kurt Bittel, *Arch. Anz.*, LV, 567-75.

(Hanyeri)

About half a mile north of the village of Hanyeri and the border of the Adana and Kayseri provinces, a Hittite rock sculpture depicts a large male figure carrying a bow and a staff or spear with a sword in scabbard at the waist. Accompanying the large figure are Hittite hieroglyphs and smaller pictorial representations. The monument is dated to the fourteenth to thirteenth centuries B.C.

Arch. Anz., LV, 560-64. Now published by Ali Rıza Yalgin in *Türk Tarih, Arkeologiya ve Etnografya Dergisi*, Vol. IV (1940).

(Maraş)

This site of many late Hittite monuments has yielded another, a stele about forty inches high. The stele probably represents a god and goddess whose arms are about each other in an unusual position. It is to be dated probably not earlier than the eighth century B.C.

Arch. Anz., LV, 564-66.

(Mersin). Neilson Expedition of the University of Liverpool

See previous *AJSL* reports and John Garstang, "Excavations at Mersin, 1938-39," and Miles Burkitt, "The Earlier Cultures at Mersin," *AAA*, XXVI (1939/40), 38-72.

(Pancarlı)

A damaged orthostat was found here which depicts a man holding a lion in his left hand and a double ax in his right. Style and clothing date this monument to the beginning of the first millennium B.C.

Arch. Anz., LV, 566.

Tarsus (Gözlü Kule). Bryn Mawr College, Archaeological Institute of America, and Fogg Museum of Harvard University

Hetty Goldman, "The Sandon Monument of Tarsus," *JAOS*, LX (1940), 544-53; Florence E. Day, "The Islamic Finds at Tarsus," *Asia*, March, 1941, pp. 143-48.

Thrace

Report by Arif Müfid Mansel on excavations carried on by Turkish expeditions during 1936 and 1939 in Thrace, *Türk Tarih Kurumu Belleten*, IV, Part XIII (1940), 115-39.

(Topkapı Sarayı)

Report by Aziz Ogan on excavations carried out in 1937, *Türk Tarih Kurumu Belleten* IV, Part XVI (1940), 329-36.

IRAQ

General

Fr. W. von Bissing, "Ägyptische und ägyptisierende Alabastergefäße aus den deutschen Ausgrabungen in Assur," *ZA*, XLVI (1940), 149-82; Th. A. Busink, *Sumerische en Babylonische tempelbouw* (Batavia-Centrum, 1940); L. Legrain, "Nippur Again. Sumerian Heads: Archaic Engraved Stone Plaque," *UMB*, IX (1941), 9-14; Heinz Lenzen, "Die Zikurrat in Ur," *ZA*, XLVI (1940), 116-48; E. A. Speiser, "The Beginnings of Civilization in Mesopotamia," *Antiquity*, XV (1941), 162-75; R. F. S. Starr, "A Rare Example of Akkadian Sculpture," *AJA*, XLV (1941), 81-86.

Eshnunna (Tell Asmar)

Max Hilzheimer, *Animal Remains from Tell Asmar*, translated by Adolph Brux ("OIC," No. 20 [Chicago, 1941]).

Nuzi

G. R. Driver and Sir John C. Miles, "Ordeal by Oath at Nuzi," *Iraq*, VII (1940), 132-38; E. R. Lacheman, "Nuzi Geographical Names. II," *BASOR*, No. 81 (February, 1941), pp. 10-15; "Nuziana," *RA*, XXXVI (1939), 81-95; and *Miscellaneous Texts* (Joint Expedition with the Iraq Museum at Nuzi, American Schools of Oriental Research, "Publications of the Baghdad School," *Texts*, Vol. VI [New Haven, 1939]).

IRAN

General

Henry Field, *Contributions to the Anthropology of Iran* (Field Museum of Natural History, "Anthropological Series," Vol. XXIX, Nos. 1-2 [Chicago, 1939]); *International Congress of Persian Art and Archaeology*, 3d, Leningrad, 1935 (*III^e Congrès international d'art et d'archéologie iranienne, Mémoires, Leningrad, septembre 1935* [Moscow, 1939]); Arthur Upham Pope and Phyllis Ackerman, "New Discoveries in Iran Exemplify the Superb Art of Persian Goldsmiths over a Period of Seventeen Centuries," *ILN*, May 31, 1941, pp. 718 f.; E. F. Schmidt, *Flights over Ancient Cities of Iran* (special publication of the Oriental Institute of the University of Chicago [Chicago, 1940]); Sir Mark Aurel Stein, *Old Routes of Western Iran* (London, 1940).

Luristan

Arthur Upham Pope and Phyllis Ackerman, "Prehistoric Nature Worship in Western Iran: Bronzes from Kuh-i-dasht," *ILN*, March 1, 1941, pp. 292 f.

Persepolis (Takht-i-Jamshid)

A fragment of a limestone relief from Persepolis is published in *UMB*, IX (1941), 28.

Shāpūr. Loure and Ministère de l'Education Nationale

R. Ghirshman and Mme Ghirshman continued working at this site during the winter of 1939/40 and returned for another campaign during the winter of 1940/41. Early in 1941 it was reported that they had discovered several meters of mosaics with portrait heads on panels and dancing figures and girls with musical instruments on other panels. These are the first Sassanian mosaics discovered. No further details are available.

AJA, XLV (1941), 165, and private correspondence from R. Ghirshman.

(Yazd-i-Khwast)

Myron B. Smith, "Three Monuments at Yazd-i-Khwast," *Ars Islamica*, VII (1940), 104 f.

INDIA

General

Bedřich Hrozný, *Über die älteste Völkerwanderung und über das Problem der proto-indischen Zivilization* (Československý orientální ústav v Praze, "Monografie archivu orientálního," Vol. VII [Prag, 1939]); M. S. Vats, *Excavations at Harappa between the Years 1920-21 and 1933-34* (2 vols.; Calcutta, 1940).

U.S.S.R.

General

Considerable information on recent widespread archeological activity throughout the Soviet Union is contained in the following summaries and reports: Henry Field and Eugene Prostov, "Archaeology in the Soviet Union," *Antiquity*, XIV (1940), 404-26; "Archaeology in Uzbekistan," *Antiquity*, XV (1941), 194-96; "Excavations in Uzbekistan," *Asia*, May, 1941, pp. 242-44; *AJA*, XLIV (1940), 535 f.; *AJA*, XLV (1941), 112-15, 299-301.

BOOK REVIEWS

SMITH, J. M. POWIS. *The Prophets and Their Times*. Revised edition by WILLIAM A. IRWIN. Chicago: University of Chicago Press, 1941. Pp. xvii+342. \$2.50.

It is assuredly a tribute to the late J. M. Powis Smith that after sixteen years his work on the Hebrew prophets is still in such demand that the publishers have issued this revised edition. The choice of reviser naturally fell on his former student and successor, W. A. Irwin. In a book whose subject matter is more theoretical than factual it was a delicate task that Irwin had to perform, but one that he has done exceptionally well. It would seem to the reviewer, however, that he should have completely revised the biblical quotations himself, or else he should have incorporated Smith's own revisions as found in *The Bible: An American Translation* (1931 ed.), particularly in view of the fact that he has revised the notes to bring them up to date with modern scholarship. For example, in Zech. 13:6 we know now from the Ras Shamra texts that *bēn yādhakhā* means "on your back," not "on your hands," and the last clause in Amos 7:12 should be interpreted as adverbial: "O seer, go away; off with you to the land of Judah and there eat bread [i.e., earn your living] by prophesying there." One wonders, too, why the new quotation on page 80 (Hos. 11:1-9) should be set up as prose when it is clearly poetry. Also, the quotations on page 4 should have "you" in place of "thou" to make them consistent with the rest of the volume. This is a slip carried over from the first edition.

A few other random observations may be noted. On page 13, note 39, Irwin should have included the discussion of prophecy favorable to his point of view by Porteous in *Record and Revelation*, edited by H. W. Robinson (1938). One is a bit shocked to have Torezyner set down as the authority on the Lachish letters (p. 186, n. 37) and to have Jack, Dussaud, and Schaeffer cited as the authorities on the Ras Shamra texts (p. 234, n. 31), when their views need to be corrected by those of their critics (see, e.g., Albright, *BASOR*, No. 82, pp. 18 ff.; No. 71, pp. 35 ff.; Bea, *Biblica*, XIX, 435 ff.; XX, 436 ff.). It would seem useless to cite a book like Harper's *Assyrian and Babylonian Literature* (1901), because it is so obsolete and has long been out of print. To our discredit there are no really good translations of cuneiform literature in English. Luckenbill's is the best translation of the Taylor prism of Senacherib that we have, as noted on page 109, note 12, but it is far from perfect. On page 138 one wonders why Irwin did not follow his own emendation of Jer. 1:13 as presented in *AJSL*, XLVII, 288 f., or, better still, an emendation

(*ûphânā(y)w* for *wnōphhō*) based on a suggestion of Ehrlich (see the reviewer, *JQR*, XIV, 285, n. 23). The final *hē* which is queried on page 138, note 13, is simply the terminative *hē*, as explained by the reviewer in *JAOS*, LX, 230. On page 219, note 2, Olmstead should have been given the credit for being the first to attach Isaiah, chapter 35, to Second Isaiah (see his note, *AJSL*, LIII, 251). Only one typographical error was noted: "Shemiaiah" (p. 35, n. 7).

Irwin has set himself against Smith's view that the canonical prophets were in any way ecstatics, but on page 12 he retains Smith's statement that there was "no sharp break between the early prophets and the great prophets of later times." As a matter of fact, Irwin sees a great difference between the two, apparently something like the change in Mohammed after establishing himself in Medina. Any ecstasies supposedly experienced by the later prophets are to be interpreted as nothing more than a literary device. But Irwin labors under the delusion that the ecstatic experience of God (which incidentally need not express itself in trances) makes a man less rational. This was assuredly not the case with Mohammed, whose earlier utterances, based on actual ecstatic experiences, are much superior to those of the later period, when he followed a literary device which earlier experience had shown him to be effective. The professional prophets can be accused of following a literary device, and perhaps a prophet like Ezekiel, but scarcely the prophets of the eighth century. In his *Hebrew Origins* (1936) the reviewer has devoted a whole chapter to Hebrew prophecy, and he would have been glad if Irwin had given consideration to the thesis there propounded that the early prophets represent a protest against the professionalization of the priestly office, while the later prophets grew out of a similar protest against the professionalization of the prophetic office. On Irwin's theory it is difficult to explain the fact that the utterances of the prophets, like those of Mohammed in his earlier career, were short, direct, authoritative, and only slightly argumentative—not long-drawn-out, reasoned, and logically arranged sermons, but oracles, full of conviction as coming from Yahweh and surcharged with the white heat of emotion. As Amos put it (3:8), "The Lord Yahweh has spoken; who will not prophesy?"

The most original part of the book is the chapter on Ezekiel. Here nothing remains of Smith's treatment except one citation. This is revision with a vengeance, but it is symptomatic of the tremendous change in critical opinions since the days of Smith. As Irwin well says (p. ix), the criticism of Ezekiel stands at present in complete chaos, the only agreement being that the book is composite and that Ezekiel began his ministry in Palestine. Under these circumstances Irwin is perfectly justified in presenting his own interpretation of the book, but its complete exposition and vindication are reserved for a later, more appropriate, publication. In the present volume he could only present his conclusions in the baldest outline. The Book of Ezekiel, according to him, was not the father of Judaism, as it was with Smith, but the child of Judaism (p. 216). The dates in the book are of doubtful genuineness (p. 208), and he

rejects most of them. The genuine oracles of Ezekiel are in the main poetic (p. 212). Not only are whole chapters spurious (e.g., 40-48), but whole sections throughout the book, some of which (e.g., 11:17-20; 20:33-38, 40-42; 34:13-16; 36:24-38) come from the hand of a gentle and pious Jew of the Greek period or later (p. 204), while others (e.g., 4:13; 5:10, 12; 12:15-16; 20:34-38,¹ 41; 22:15) originate with a still later commentator whose mood was stern and denunciatory (p. 205). In fact, the book is interspersed with the comments of numerous writers, and Irwin professes to be able to separate the wheat from the chaff. Judgment on the thesis must await his more elaborate presentation, but in the meantime it is attractive enough to whet our appetite for its fuller elucidation.

To a less degree the interpretation of Hosea has been re-written. Irwin argues that the Gomer of chapter 1 was a religious prostitute belonging to the fertility cult and that the adulteress of chapter 3 was a different woman, the two chapters being records of two distinct incidents in the prophet's career (p. 74). There were two marriages because Hosea had two different object-lessons in mind. We heartily commend Irwin's treatment as representing a decided advance over that of Smith.

The rest of the volume follows more nearly the lines laid down by Smith. The discussion is brought up to date and all the more important later views are taken into account, to make a volume seventy-four pages longer than its predecessor. We predict for the book a reception as cordial as that accorded the first edition and many years of usefulness as an authoritative guide to the prophets and their times.

THEOPHILE J. MEEK

University of Toronto

VAN BUREN, DOUGLAS E. *The Cylinder Seals of the Pontifical Biblical Institute*. ("Analecta orientalia," No. 21.) Rome, 1940. Pp. xii+51+12 pls. Rm. 14.

The Pontifical Biblical Institute bought this collection of cylinder seals for the use of students. Its variety and comprehensiveness, consequently, were more important than the quality or special features of individual pieces; but the seals, though of the usual types, warrant publication. The author brings her wonted enthusiasm to the task, and the text of the catalogue shows a praiseworthy attempt to counteract, by lively prose, the dullness inherent in a string of unconnected descriptions. It is probably a matter of taste whether one prefers this method or its opposite, namely, the reduction of the descriptive text to the absolute minimum in which nothing is insisted upon which the picture shows clearly. After a discussion of eighty-seven genuine pieces, the author pictures and describes about twenty which she considers forgeries. It

¹ There must be some slip here because vss. 33-38 of this chapter have just been assigned to quite a different hand.

is disquieting to note how many can be traced back to prototypes in well-known collections—some of which may have been imitated when still in dealers' hands, while others seem inspired by some such scholarly work as that under review. At any rate, the treatment of these forgeries is highly instructive and embodies much painstaking and conscientious work.

The reviewer is not quite convinced of the genuineness of No. 7, though no suspicion can take definite shape on the basis of an illustration only. The seated figure on this seal excludes in any case the Early Dynastic period to which it is assigned; if genuine, the seal belongs to the First Babylonian Dynasty; the drillholes point in the same direction,¹ but the figure holding the gateposts combines the head of the "nude hero" with the legs of the bull-man—a confusion of motives hardly to be expected from an ancient seal-cutter.

The outstanding piece in the collection is No. 14. It shows the killing of the "Bull of Heaven," symbol of drought; and, furthermore, the dragons of the weather-god which stand for the storms with their thunder, lightning, and life-giving rain.² This interpretation is quite certain if one considers our seal in connection with other renderings of the subject, but the author unfortunately has not recognized its true meaning. She interprets the bull as a sow, perhaps because the hunting dog³ which assists the slaying god is considered by her as a piglet. This small figure certainly lacks clarity; but the victim of the god is not a boar or a sow, as comparison with indubitable renderings⁴ will show. Moreover, our author is compelled by her interpretation of the animal to see in the bull's horn "a curved implement" held by the god. But the only implements depicted on Akkadian seals are weapons or identifying attributes. Bullfighters on Akkadian seals often grasp the horn of the beast, and, in fact, exactly this feature occurs in the Hermitage rendering of our subject.⁵ It is true that the neck of the bull on the seal in Rome shows a curious "ruff" which may be due to a chipping of the seal or to careless tooling in the engraving of the neck. But the tail as well as the comparisons noted above exclude the interpretation as a sow.

Some details in the scene remain inexplicable, notably the role played by the two figures placed among the dragons. One of these men or gods seems to release a dragon for its headlong flight toward the earth; the oblique placing of the dragons on our seal and on the Hermitage example may actually be due to their symbolizing the lightning and slashing rains of the storm. In a seal from the Pierpont Morgan Library⁶ the dragon pulling the god's thunder chariot spits fire. But, in any case, whatever may be the exact interpretation

¹ Frankfort, *Cylinder Seals*, Pl. XXVI d, f, and p. 148.

² *Ibid.*, pp. 124–27.

³ Or possibly lion, depicted on the small scale of an attribute.

⁴ Frankfort, *op. cit.*, Pl. XXIII h, i.

⁵ Ward, *Cylinder Seals of Western Asia*, No. 129 a.

⁶ Frankfort, *op. cit.*, Pl. XXII a.

of certain details, the meaning of the scene as a whole is well established not only because this type of dragon is, in Akkadian times, the exclusive attribute of the weathergod and his spouse but also because the subject is very explicitly depicted on a seal in the British Museum where the death of the bull is shown to coincide with the breaking of the storm.⁷ There is no reason to claim that the scene takes place in "wild mountainous country"; its significance lies precisely in the value of rain for the peasants in the Plain of the Two Rivers, and the presumed "rocks" seem due to accidental chipping of the edge of the cylinder. Mountains, in any case, are rendered in Akkadian times by a regular scale pattern.

The errors into which so experienced a student as Mrs. Van Buren has fallen in this case emphasize once more the impossibility of interpreting any ancient seal in isolation. The first question must always be to which group of representations a given design belongs; next it must be determined which meaning the combined testimony of all versions suggests for the underlying myths or stories; and only after that may it be attempted to interpret any given variant of the subject.

The other seals in the collection do not call for comment. It remains to state that the author and the authorities of the Pontifical Biblical Institute have placed us under a real obligation by the publication of this catalogue.

H. FRANKFORT

*Oriental Institute
University of Chicago*

STARR, RICHARD F. S. *Indus Valley Painted Pottery*. ("Princeton Oriental Texts," Vol. VIII.) Princeton: Princeton University Press, 1941. Pp. xiii + 106 + 1 map. \$3.50.

In this study Dr. Starr has undertaken the worth-while task of determining the status of the painted pottery of the Harappa culture in relation to the "great painted pottery family of Western Asia" (p. 6)¹ and thereby elucidating the relationship of the Indus Valley to other portions of the ancient world.

The reviewer is in considerable disagreement with the method used by Starr and with his conclusions. His method of comparing designs does not seem to lead to the most certain results possible; the assumption of a fairly close relationship between the Harappa, Amri, and Baluchistan cultures, which is the only justification for many of his East-West comparisons, does not seem justified; and particularly the absence of a chronological treatment of the Baluchistan and Indus Valley remains appears inevitably to vitiate Starr's conclusions. In fairness to him, however, his method and conclusions

¹ *Ibid.*, Pl. XXII e.

² The page references are to the book under review. The abbreviations used for other references are those employed by Starr.

are presented in the following three paragraphs before they are critically examined.

In Part I the various types of pottery to be utilized are described and discussed. These include, in the West, the early Mesopotamian and Iranian painted pottery; in the East, painted pottery from Baluchistan and the Indus Valley. Some indications of a Baluchistan sequence of cultures are given, but the relative order of the Baluchistan remains is not considered certain. Nevertheless, owing to an "obvious interrelationship" of Baluchistan and Harappan pottery, "various Baluchi specimens of painted design" of the types at Kulli, Mehi and Periano-ghundai are used in comparison with the West "not as substitutes for Harappan examples, or as their equivalent, but as representatives of the broad eastern family of which Harappa is a member" (p. 20). The Amri culture in the Indus Valley is recognized to be the direct predecessor of the Harappa culture. Because of the similarity in design between the pottery of the Amri and Harappa cultures, the former is used in the "comparison between Harappa and the west more frequently than any other of the Indus and Baluchistan fabrics" (p. 23). Starr recognizes the great time interval between the pottery of the West and Harappa. He infers, however, that, because of the static character of Harappan pottery, we may consider that Harappan "pottery decoration was a tradition long-fixed and faithfully retained, thus carrying our Harappan products back close in time to the prehistoric Iranian and Mesopotamian wares" (p. 11).

In Part II Starr proceeds to compare pottery designs of the Indus Valley and Baluchistan with the early painted pottery of Iran and Mesopotamia. Consideration of the pottery fabric and forms and a chronological treatment are excluded, and the comparisons are mainly of design elements and not design composition or style. In general, the method followed is to discover the possibly natural, i.e., representational, prototype of a design, or its basic geometric form if a representational origin cannot be found. Once the basic form of a design is recognized, variant types are distinguished which are then used in the eastern and western comparisons. The variant forms of the same basic motive may appear quite dissimilar but are compared because they are derived from the same original geometric or representational element.

From a study of twelve basic elements or patterns and their variants, as well as plant and animal designs, Starr concludes that "except for the plant motives, a few of the animals, and a queer" element, "there is not a single decorative element, not one pattern or motif, that does not have a correspondent among the earlier cultures of the west" (p. 87). Despite the western elements used, the style, or the impression which the design as a whole gives, is recognized to be individual and Harappan. Starr feels certain that the elements shared by Harappa with the West are an inheritance from the older cultures of Iran, Elam, and Mesopotamia and doubts if this similarity is due to borrowing alone. He reaches this conclusion because there is "no closely comparable ware from India or Baluchistan that is clearly and demonstrably

older than Harappan" (p. 87);² because the heaviness of the Harappan design and its standardization point to a conscientious retention of a much earlier decorative style; and because the marked difference in the content of the Harappan pottery design (which is in a tradition essentially foreign to the Indus) and that of other Harappan art indicates different traditions for the ceramic and nonceramic art. These facts are explained by the presence of two racial elements in the Harappan civilization. One derived from the West and insured the survival of the peculiar technique of pottery decoration; the other, responsible for the nonceramic art, was "native to the Indus," though not necessarily "autochthonous in India." By the time these people are known from the earliest discovered Harappan remains, they were amalgamated into a homogeneous stock. The presence of the western tradition is the result of inheritance, and the Harappa civilization is not the direct descendant of any western culture, for Starr would postulate a mixed and remote cultural and racial connection. The western element in the Harappan pottery design is not equated exclusively with any one western culture, though it is related to them collectively as a single cultural family. Starr finds it likely, however, that "an appreciable number of people brought up in the artistic tradition of the Halaf culture went into the make-up of the mixed race which was to evolve as Harappa" (p. 99).

Within the limitations with which his study is circumscribed, Starr has used all the material available. He shows a truly inquiring attitude and is never dogmatic in regard to the problems involved. The reviewer is in agreement with his view that there is a considerable Iranian tradition in the ceramic design of the Baluchistan and Amri cultures and, to a lesser extent, in the Harappa culture.

Starr's study would be more convincing if he had not limited himself to pottery design. Ceramic design is very valuable for comparative purposes, but conclusions as to the relationship of cultures are always more convincing the larger the number of traits shared by the civilizations which are to be compared. On the basis of a nonchronological comparison of ceramic design it seems to me that Dr. Starr has exceeded his evidence when he assumes that the western element became a part of Harappan culture before the latter was settled in the Indus Valley. In doing this, he is drawing conclusions of chronological significance from a nonchronological study of his material, while supporting them with an interpretation of a purely stylistic fact, that is, that Harappan design is static. An exhaustive study is needed of all the traits of the Harappan culture in relation to the cultures of Baluchistan, Amri, and the earlier Iranian cultures, before such far-reaching conclusions can be accepted.

Evidence for distinct elements in the Baluchistan and Indus Valley cultures might have been found if the author had been less skeptical of the value

² I am not quite certain what this means, for the Amri culture is admitted to be the predecessor of the Harappan. Apparently, this refers to an earlier stage of the Harappan culture when it had not yet settled in the Indus Valley.

of pottery fabric as a good criterion for comparison (pp. 7, 22, 90-91). A few words need to be devoted to this question, for it is unlikely that the use of a red slip "merely denotes a regional or group preference shared at random by east and west alike." Painted red ware, slipped or plain-surfaced, does not appear in Mesopotamia before the Uruk period. In Iran the situation is somewhat more complex. Painted red and buff wares do occur together in Siyalk I, but there is reason to suspect that they represent distinct cultures. Painted red ware alone occurs in Siyalk II. In Siyalk III and Hissar I, red ware is gradually replaced by buff ware as a result of influence from the buff-ware culture (Giyān V, Susa I, Bakun A).³ In all these cases the types of design used correspond to the ware on which they were applied.⁴ I believe that a certain degree of relationship between design and ware can be shown for the Baluchistan and Indus Valley pottery. The Amri and Nal cultures, which have striking design associations with Iran, seem to attempt to achieve in the main a light-surfaced ware; the culture of Mehi and Kulli and that of Harappa prefer red burnished surfaces and seem basically non-Iranian. At any rate, in every case in which a culture or cultures use pottery of different wares, a detailed study should be made to see if there is any possibility of correspondence between designs and wares.

In Part II Starr's comparisons of western and eastern designs seem to be confused in part by the attempt made to discover the original geometrical or representational prototypes. Subjective considerations certainly enter into the delicate matter of comparing variant forms of a design, with the result that such evidence is of secondary value. Many of the comparisons made in this book are of this sort and are therefore of doubtful validity. As long as a geometrical motif can be traced back to a representational original, it is the more probable that the same geometric form could arise independently in various areas. The more peculiar and complex the elements compared, whatever their origin, the surer we can be of a connection and the less likelihood there is of separate invention.

A number of designs compared with the West are not found in the Harappa culture but only in the cultures of Baluchistan and Amri. These are of dubious value, therefore, as regards the direct relationship of Western and Harappan design. Starr uses such comparisons because he believes that the painted pottery of the cultures of Baluchistan and the Indus Valley all belong to an eastern painted-pottery culture. This seems an unjustifiable assumption. Before we can consider the ceramic design of the Baluchistan and Amri cultures as basically Harappan (pp. 20, 21), their chronological relationships must be es-

³ The conclusions which are expressed here as to internal Iranian and Iranian-Mesopotamian relationships are based on a study of the relative stratigraphy of early Iran which will soon appear as *SAOC*, No. 23.

⁴ In Siyalk III and Hissar I the same designs are used on red and buff wares during the transitional phase only.

tablished and a detailed study of likenesses and differences be made. Had this been done, certain valuable material could have been utilized which would have inevitably modified Starr's conclusions. The position of the Nal culture is of real importance, for in it are found certain designs which are very close to the prehistoric designs of Fars in Iran. Fortunately, from two of Majumdar's excavations in Sind—at Pandi Wahi and Ghazi Shah—there is clear stratigraphic evidence that the Nal culture is older than the Harappan.⁵ Once we appreciate this fact, the explanation of Iranian elements in Harappan design becomes much clearer. There is no need to postulate contact between originally Iranian cultures and the Harappan civilization much before the beginning of the Early Dynastic period, when the latter was established in the Indus Valley. The Nal and Amri cultures contained an Iranian element from which this element in Harappan derived. This in no sense implies that the Nal or Amri cultures were ancestral to the Harappan. It merely indicates that during the period of transition between these cultures certain design elements and patterns were taken over by the Harappan ceramic industry to be rendered in its own peculiar style.

We thus see that—in contrast to Starr's view—there are earlier cultures than the Harappan in Baluchistan and the Indus Valley. Furthermore, there is no necessity to assume that the static character of Harappan design means it derives from a very old tradition. It seems quite as probable, or more so, to postulate an earlier stage of the Harappa culture than is known in the Indus Valley, in which red and gray pottery was unpainted. When the Harappa culture settled in the Indus Valley, it was in contact with the Amri and probably Nal cultures, from which was borrowed the idea of painting pottery as well as some elements of design. The native plants and animals were also drawn, and the whole was given a distinct style. This also explains the rarity of painted pottery in the Harappa culture.

A few particular points may be mentioned. The sherd shown in Figure 89 was found with Harappan pottery, but—as pointed out in our discussion of the Ghazi Shah stratigraphy—this same design is found at -39' with Amri pottery only. At Ghazi Shah, therefore, it continued in use at the beginning of the Harappa period. Originally this particular design derived from the Nal culture. Figures 163 and 165 are considered to be Harappan; actually they

⁵ At Pandi Wahi (*Sind*, pp. 109-14) in Trench I below +1', unmixed Amri pottery was found. At +2' a typical Nal design occurred (*ibid.*, Pl. XXVIII:8), and at +3.2' more Nal sherds appeared (*ibid.*, Pl. XXVIII:18, 22). The first Harappan pottery is shown from +8-10'. At Ghazi Shah, Amri pottery was found below -35', while the first Harappan design is shown from -32.3'. At -39.2' a Nal design was found (*ibid.*, p. 99, G.S. 253-54; cf. *Gedrosia*, Pl. XXIII:Nal. 9, 10). At Tando Rahim Khan (*Sind*, pp. 86, 103-5) two Nal sherds (*ibid.*, Pl. XXX:26, 40, 42) are shown with Amri pottery, and no Harappan pottery was found. More space cannot be devoted to the study of the stratigraphy of Pandi Wahi and Ghazi Shah, but both sites show evidence of the appearance of typical pottery design of the type found at Kulli and Mehi with the earliest Harappan pottery, and of a transition between the Amri and Harappa cultures.

were found at Ghazi Shah at -37' in a level at which only Amri pottery was found.

Figure 24 is not related to the fringed loop pattern, for it shows an "ibex" with wavy horns. Compare the long-legged bulls from Moghul Kala and the crowded form of this design at Sur Jangal.⁶

There is not space to discuss Starr's table (p. 8) of the relative position of the Mesopotamian periods and the levels of Iranian sites. It differs from Schmidt's views⁷ and from those of Ghirshman⁸ as well as from my own and, therefore, until publication of the evidence on which it is based, must be considered as highly tentative, as Starr himself does.

Starr's use of the term "great painted pottery family of Western Asia" requires some comment. Enough is known of Mesopotamia and Iran so that it is clear that the cultures with painted pottery cannot be treated as belonging to one original family. Two large families of light-faced wares are well known: that of Halaf and the buff-ware culture of Iran, to which the Samarra and Ubaid cultures were basically related. A third distinct group is represented by the culture of Siyalk II. The painted red pottery of Siyalk I is basically that of Siyalk II. The buff pottery of Siyalk I may represent a separate family or be related to the buff-ware culture. The pottery of Siyalk III and Hissar I is not a distinct fabric but represents a fusion of a Siyalk II type of tradition with the buff-ware culture. Thus we may distinguish at least three distinct painted pottery families in Western Asia, not just one.

Starr's explanation of the Halafian element in Harappan design also seems a little extreme. There is sufficient evidence to indicate that at a time corresponding to the beginning of the Ubaid period there was considerable Halafian influence throughout Iran. This was probably a result of the interaction between the Halaf and the incoming Ubaid culture while the latter was still in communication with the regions in Iran from which it derived. Certain Halafian traits, including some pottery design, were taken over in Iran and used in typically Iranian fashion. At a time roughly contemporary with the beginning of the Uruk period the cultures of northeastern and of western and southwestern Iran were displaced by cultures with plain, undecorated gray or red pottery. Some peoples of the cultures with painted pottery, as a result, apparently migrated to the east where we first find them in place after a considerable lapse of time in the Amri and Nal cultures. It was through this movement that Mesopotamian elements came indirectly and purely Iranian elements came more directly to the Indus Valley.

DONALD E. MCCOWN

Oriental Institute
University of Chicago

⁶ North Baluchistan, Pl. XIII M.K.2; Pl. XX S.J.32, S.J.11.1.

⁷ Hissar, p. 320.

⁸ Siyalk, pp. 90-117.

BOOKS RECEIVED

"ISHMERAI." *Ezekiel Speaks Today: A Critical Analysis of the Prophecy of Ezekiel in the Light of Modern Events*. Boston: The Writer, Inc., 1941. Pp. xiii + 647. \$2.00.

Following the suggestion of "Swedenborg and others" that "beneath the ordinary words in the Bible are often spiritual values whose discovery makes the meaning clearer," the pseudonymous author of this huge work undertakes an interpretation, chapter by chapter, of the entire Book of Ezekiel.

"Such simple words as 'water,' or 'to eat,'" he tells us, are sometimes to be interpreted by the "spiritual concept they stand for"; otherwise the passages containing them will be incomprehensible. The method of discovery of this "spiritual" interpretation the writer happily makes clear to us. In Ezek. 34:23 "we read, 'I will set up one shepherd over them, and he shall feed them, even my servant David.' We naturally understand that 'David' here symbolizes the Messianic Christ, for the old 'David' was long since dead and to none but the Christ would such leadership belong. Again it is clear that the 'feeding' is the spiritual teaching we are to have through him. So we have gained two words to be tried whenever the literal significance appears in fault."

The application of this method to Ezekiel we may sample by the treatment of chapter 29. After noting the views of certain expositors (which satisfy him that something mysterious lies beneath the innocent words of the chapter), the author invokes a "spiritual 'Egypt'" which "may be quite as distinct an object of study as a geographic one," a view that is then bolstered by reference to Isa. 30:9 and 51:9. A little further study reveals the conclusion that the "dragon" of the chapter is likewise "spiritual," as is also the river in which it lies: it is some "group at variance with the Lord Jehovah, though not cut off from the stores of knowledge." Then the scales of the "dragon," the fish that stick in them, and the hooks for its jaws are all similarly illumined by this profound method. And so on through the entire chapter. And similarly for more than six hundred pages!

And what does it signify in the end? All the "spiritual" values so laboriously brought to light—in so far as they are real—are more readily revealed by the simple method of understanding the passages in their obvious interpretation related to the thought and circumstances of their writer's age: in brief, by the critical method which "Ishmerai" purports to survey but never understands. Six hundred and forty-seven pages of unmitigated drivel! Alas for the labor, which might have been turned to some good purpose!—W. A. IRWIN.

MARRERO, LEVI. *Perfil del imperio hitita*. ("Publicaciones de la 'Revista de los estudiantes de filosofía.'") La Habana, 1939. Pp. 19.

A short sketch of the political history of the Hittite Empire based on secondary, primarily French, sources. For readers who know nothing about the Hittites, sketches of this type may perhaps be useful, but a direct translation of a good original book would have served the purpose better.—I. J. GELB.

LUTZ, HENRY F. *A Neo-Babylonian Debenture and A Recorded Deposition concerning Presentment for Tax Payment*. ("University of California Publications in Semitic Philology," Vol. X, Nos. 9 and 10.) Berkeley, Calif.: University of California Press, 1940. Pp. 251-56 and 257-64. \$0.25 each.

Copies, photographs, transliterations, and translations (but no philological notes) of two Neo-Babylonian legal documents, one dated in the time of Neb-

uchadnezzar II, the other in the time of Nabonidus. Contrary to better practice, diacritic marks distinguishing various homophonous signs are omitted in the transliterations. In line 10 of the first text the copy and transliteration as *amēlāti ušabši* instead of the required *amēlūmukinnū* are hardly possible, since *šubšū* does not mean "to cause to appear" but "to cause to be" or "to create." Instead of *amēlšangū*, "the priest," translate "the scribe," as so often in Neo-Babylonian documents. The transliteration and translation of lines 12 and 13 in the second text do not seem convincing.—I. J. GELB.

LAUTNER, JULIUS GEORG. *Altbabylonische Personenmiete und Erntearbeiter verträge*. ("Studia et documenta ad jura Orientis antiqui pertinentia," Vol. I.) Leiden: E. J. Brill, 1936. Pp. xx+262. 10 guilders.

This study is a careful and exhaustive research in the legal terminology of economic texts from the First Dynasty of Babylon concerning the hire of persons for labor. Professor Lautner has consulted all pertinent texts within the limits he has set for himself and at times refers to similar materials from other periods. He has ordered his texts into three major groups, but he correctly recognizes that not all documents fall within these categories and that there are combinations or mixed types. His detailed investigation clarifies problems connected with the payment of wages and the position occupied by employer, employee, and contractor. In this connection the apprentice documents from the Chaldean and Persian periods might have been utilized for comparative purposes. In the tradition of Koschaker, the emphasis in Professor Lautner's volume is on an understanding of the legal side of the documents rather than on their interpretation from economic and social angles.—W. H. D.

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